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Cash Transfer Programmes and Conditionality: Why Not a Lump-Sum Payment Trusting the Poor?

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PhD

Social Policy

The University of Edinburgh

2019

Declaration of own work

I declare that this doctoral thesis for the degree of PhD in Social Policy has been composed solely by myself and that it has not been submitted, in whole or in part, for any other degree or qualification. Except where states otherwise by reference or acknowledgement, the work presented is entirely my own.

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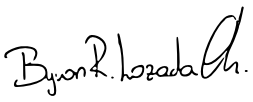	20/04/2019

Word count: 108,967.

Lay Summary

Conditional cash transfer (CCT) programmes have in recent years become a very popular instrument for the alleviation of poverty and vulnerability in developing countries. CCTs address the lack of investment in human capital among those in poverty by providing cash benefits that are conditional upon households complying with a number of specific behavioural requirements, such as sending children to school and/or bringing them to health centres for regular check-ups. However, the actual role played by the behavioural conditions in CCTs is still a topic of widespread debate in academic and policy-maker circles. In this thesis, I take advantage of the social assistance context in Ecuador to compare the effectiveness of a traditional CCT programme (*Bono de Desarrollo Humano*) and an alternative anti-poverty programme that provides accumulated payments without any attached conditions (*Crédito de Desarrollo Humano*). The results suggest that the impacts of the unconditional cash transfers are markedly superior in terms of welfare promotion, human capital investment and income generation. Overall, the findings of this research raise serious doubts about the practical importance of behavioural conditions in anti-poverty programmes.

Acknowledgements

I would like to thank my two supervisors, Prof. Alison Koslowski and Dr Daniel Clegg for their invaluable guidance and continuous support during this rewarding learning process. I could not have wished for two more suitable and prepared supervisors for whom I have the deepest respect and admiration, not only for their professional but also human quality. I hope to keep in touch with them despite the distance between our countries and that one day during my professional career I can collaborate with them in some research project. That would be a great honour for me.

My eternal gratitude to the following people for their academic and personal support during the time I was in Edinburgh completing my doctoral studies.

- Dr. Sarah Hill for her honest concern for my health when I went through difficult times and for being present to give me her wise advice.
- Dr. Elke Heins for her interest in my research, her timely advice and for always being willing to give me a hand when I need it.
- All PhD students I had the pleasure of meeting and were always around to offer an opinion or just a nice chat.

I am also ever grateful to my external and internal examiners Prof. Armando Barrientos and Dr. Jean-Benoit Falisse for the important feedback and valuable suggestions they provided me with during the viva voce examination.

I also thank the Ministerio Coordinador de Desarrollo Social (MCDS), specially to Mr. Reinaldo Cervantez and Mr. Fernando Galvez, for their timely collaboration and willingness to provide me all the information and data necessary to carry out this investigation. If it were not for the openness and generosity of this institution, since the moment I started this project, nothing would have been possible. I am also grateful to the Secretaria Nacional de Estudios Superiores Ciencia y Tecnología (SENESCYT) for their financial support during my postgraduate studies.

Most importantly, I would like to thank my family for always supporting me emotionally despite of my physical absence at home, and for backing me during important life-changing decisions, such as coming to Edinburgh to undertake this wonderful journey. Finally, I would like to thank my aunt Maria for inspiring me and being for me an example of life that now watches me from heaven.

Abstract

Most developing countries have introduced large-scale programmes of direct transfers with the intention of reducing poverty and vulnerability. Most of these programmes deliver monetary resources to poor households, provided that they comply with certain conditions associated with improving the human capabilities of their children. The dominant political discourse has been that it is necessary to help the poor in decision-making in order to break the inter-generational transmission of poverty. However, the great popularity that conditional cash transfer programmes have achieved contrasts with the lack of empirical evidence demonstrating the specific contribution of conditionality. It seems that policymakers have preferred to focus on the practical benefits of conditions, rather than worrying about their actual effectiveness.

This research seeks to analyse critically conditionality in antipoverty programmes, which I consider important for the future development of improved forms of social assistance in developing countries. More specifically, it compares the effectiveness (in terms of welfare generation, human capital investment and labour supply) of two completely different cash transfer programmes offered in Ecuador: one with the typical conditional structure (Bono de Desarrollo Humano) and a recently implemented programme in which households receive a lump-sum payment without attached conditionalities (Crédito de Desarrollo Humano). For this purpose, I use different quasi-experimental methods together with two specific impact evaluation designs, which represent a significant methodological contribution to an evaluative literature where experimental methods have predominated. The analysis is based on linear, probit and ordered logistic regression models.

Theoretical concepts pertaining to social protection and social assistance in developing countries, as well as the different theoretical perspectives about cash transfers, and the arguments for the inclusion and exclusion of conditions, frame the research questions and the interpretation of results. This research draws on the household- and

individual-level data from the Ecuadorian Registro Social 2008 and 2014 databases, which compile surveys applied to families in areas with a high incidence of poverty.

The empirical analysis shows that the non-conditional programme Crédito de Desarrollo Humano (Human Development Credit) has consistently superior effects on overall welfare, the average educational attainment of the households and the housing conditions, when compared to the conditional programme Bono de Desarrollo Humano (Human Development Bonus). Surprisingly, the conditional programme has no effects (or even small negative effects) on these outcome variables. This points to the low importance of conditionality in practice, which seem to play a secondary (or even harmful) role in generating welfare among the poor, in comparison with other constituent elements of the programmes (such as the amount of transfers and complementary policies). Therefore, the evidence clearly suggests that the presence of conditionalities in social assistance is neither necessary nor sufficient to ensure that families overcome poverty in a sustainable manner reaching higher levels of well-being, increasing their human capital and improving their living conditions.

Moreover, neither programme has a significant impact on the probability of school enrolment/attendance of the first-born child. Although some statistically significant effects were estimated, their magnitudes are very small. Therefore, the findings indicate that conditionality is not sufficient to guarantee that parents invest in the human capital of their older children. Probably, conditions should be accompanied by constant awareness campaigns or better control and verification mechanisms. However, conditionality seems to be necessary for this purpose since the unconditional cash transfers had no effect on the parent's behaviour, which is by itself an interesting and important result, since there is the idea that this programme –which promotes the generation of productive enterprises– could be causing at the same time an increase in child labour.

Finally, neither of the two social assistance programmes negatively affects the probability of being unemployed. Both the beneficiaries of the Bono de Desarrollo Humano programme and the beneficiaries of the Crédito de Desarrollo Humano are

equally likely to be unemployed than poor individuals who do not receive any kind of benefit. At least in this case, welfare programmes are not generating disincentives to work among those in poverty. Overall, the analysis suggests that cash transfer programmes can achieve better results if they leave conditionalities aside and opt for the productive inclusion of the poor by linking cash transfers to interventions/policies that aim to directly increase household productivity. Through elements such as mutual trust and training, more effective and sustained escapes from poverty might be achieved.

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List of Abbreviations

ASPIRE	-	Atlas of Social Protection - Indicators of Resilience and Equity Database
ATEs	-	Average Treatment Effects
ATET	-	Average Treatment Effect on the Treated
BDH	-	Bono de Desarrollo Humano (Human Development Bonus)
CCTs	-	Conditional Cash Transfers
CDF	-	Cumulative Distribution Function
CDH	-	Crédito de Desarrollo Humano (Human Development Credit)
DID	-	Difference-in-Differences
IEPS	-	Instituto de Economía Popular y Solidaria (Institute of Popular and Solidarity Economy)
INEC	-	Instituto Nacional de Estadísticas y Censos (National Institute of Statistics and Census)
IoT	-	Intensity of Treatment
IV	-	Instrumental Variable
LATE	-	Local Average Treatment Effect
MCDS	-	Ministerio Coordinador de Desarrollo Social (Ministry Coordinator of Social Development)
MIES	-	Ministerio de Inclusión Económica y Social (Ministry of Economic and Social Inclusion)
OLS	-	Ordinary Least Squares
OVB	-	Omitted Variable Bias
QED	-	Quasi-Experimental Designs
RCTs	-	Randomized Controlled Trials
RS	-	Registro Social (Social Register)
SELBEN	-	Sistema de Identificación y Selección de Beneficiarios de Programas Sociales (System of Identification and Selection of Beneficiaries of Social Programmes)

SSB	-	Self-Selection Bias
ToC	-	Theory of Change

Introduction

Since the United Nation's Millennium Development Goals (MDGs) were designed and launched in 2000, a lot of academic and policy work has gone into trying to meet them. The MDGs have captured the world's attention on the importance of reducing poverty and vulnerability more than any other global initiative in the past (Barrientos & Hulme, 2008). As a result, during the last eighteen years, development-related institutions, poor and rich country governments and different policy researchers have emphasized the role of social protection, poverty eradication and, above all, the struggle for greater equality as key ingredients in building democratic, fair and prosperous societies (CEPAL, 2010). Specifically, they have highlighted the importance of implementing actions that, on different fronts, allow moving towards greater degrees of social inclusion, equality and respect for the principles of human rights.

Despite these significant efforts, billions of people in developing countries and emerging economies are still completely at the mercy of such risks as unemployment, poverty and illness (BMZ, 2008; UN, 2014).[1] In the past, development policy has emphasized the necessity of positive interventions in the state's supply of public goods and services by providing sufficient schools, health clinics and other basic infrastructure (Rawlings & Rubio, 2005). However, merely expansion of the supply side failed to sufficiently increase service utilization. Part of the reason for this could be that even when service costs are kept low or are removed completely, supply-side improvements tend not to be effective in modifying long-term patterns of use because resource constraints faced by poor households do not allow them to pay the basic private costs associated with the

[1] According to the UN (2014) Human Development Report, over 2.2 billion people, representing more than 15 percent of the world population, are near or in multidimensional poverty. At the same time, more than 1.5 billion people —nearly half of all workers—are in informal or precarious employment. Moreover, nearly 80 percent of the global population lack comprehensive social protection and 12 percent (842 million) suffer from chronic hunger.

use of public services, which include transportation costs and the opportunity costs of time (Maluccio & Flores, 2004).

The main challenge for the developing world today is the appropriate design and implementation of social policies that address -from a multidimensional perspective- the complex edges of exclusion, vulnerability, inequality and poverty. As part of such policies, the international community has recognized the necessity of strengthening social protection systems in those parts of the world where they do not exist or are very fragile. It is important to note that more than half of the world's population lacks reliable protection against the effects of hazards like diseases, unemployment or old age (UN, 2012).

A number of factors clearly indicate the prominence of social protection as the political framework employed to fight poverty and vulnerability in developing countries over the last two decades (Barrientos, 2010). First, more and more governments are developing and implementing social protection policies and programmes within their national poverty reduction strategies (Barrientos & Holmes, 2007). Second, international organizations (such as the World Bank and the United Nations), the European Union and different developed countries have shown their willingness to increase support in setting up adequate social protection systems in the developing world, which can help maintain a social balance, sustainable poverty reduction and social development, especially in countries where inequality is a growing concern.[2] Finally, there is also a growing interest in the issue of social protection among academics and development research institutions in general (Barrientos, 2010).

The Ecuadorian government has among its priorities the fight against poverty and inequality in the country through the implementation of different social assistance practices (MIES, 2013b). As in other Latin American countries, one of the most used

[2] For example, German development cooperation shares its experience in adequate social protection systems with about 30 partner countries throughout the world and assists them in implementing sustainable solutions for their own systems' problems (BMZ, 2008).

strategies has been the delivery of cash transfers as a mechanism to smooth consumption and promote well-being among households in vulnerable situations. My research seeks to compare the effectiveness of two completely different social assistance programmes offered in Ecuador: one with the typical structure (i.e. cash transfers attached to conditionalities) and a recently implemented programme of unconditional (and lump-sum) monetary transfers.

The Bono de Desarrollo Humano (BDH) is a monthly cash benefit of 50 US dollars designed to help those in poverty to pay for their most basic costs of living (like rent, clothes and transportation). This programme falls into the category of traditional conditional cash transfers (CCTs) because the monetary benefits are subject to compliance with conditionalities related to investing in human capital development (i.e. health and education of children). The BDH cash transfers are delivered to households (preferably to the mothers) living in extreme poverty (i.e. those in the first poverty quintile) or vulnerability. On the other hand, the Crédito de Desarrollo Humano (CDH) programme provides unconditional monetary benefits (accompanied by technical assistance) that seek to generate financing facilities for the poor, as well as to establish formal mechanisms and economic incentives to support their productive investments. The CDH cash transfers of 600 US dollars represent one year of accumulated BDH transfers. They can be requested annually only by those in vulnerable conditions (i.e. potential BDH beneficiaries, people with disabilities and senior citizens without social security).

Contextualizing this Research

The recent increased attention to the topic of social protection in the fields of social development and more generally in development studies was by all means necessary, but also somewhat belated. Although the study of social development emerged significantly in the 1950s, neither policy-makers nor academics working in the field had previously given to social protection policies and programmes the importance that they might deserve

(Midgley, 2013). These practices and policies were merely associated with improvements in consumption levels rather than activities that facilitate the socio-economic development of the most vulnerable, and therefore they were viewed in the political and academic spheres as more suitable for Western world countries.

It was commonly thought that social protection goes far beyond the primary interests of social development, which are basically related to national social planning, participatory community development programmes, poverty reduction, gender projects and improvements in nutrition, health and education (Midgley, 1995; Seers, 1969). In fact, income transfers and social security, for example, were widely understood as costly and unnecessary luxuries to which less-developed countries could not have access (MacPherson, 1982). Consequently, it is hardly surprising that the issue of social protection programmes in the developing world was given little priority in previous social development literature, since they were considered a mere consumption expenditure that diverts important resources from more appropriate interventions that require large economic and social investments.

However, the situation has changed drastically in recent years. Different social protection projects and programmes designed to help those in poverty have become very popular in the developing world (Barrientos, 2010). The introduction of direct cash transfer schemes in Latin America and Sub-Saharan Africa, the rapid expansion of social insurance coverage in Thailand, the launching of the Minimum Living Standards Guarantee Scheme in the Republic of Korea, the introduction of mixed provision health insurance systems in rural China, the creation of the National Rural Employment Guarantee Scheme in India and the introduction of the Child Support Grant as well as universal old-age pensions in South Africa have all contributed to the growing interest in the issue of social protection. Moreover, significant technical and institutional innovations to social protection practices, such as cash transfers combined with services and non-contributory social pensions, have been consistently implemented in countries as diverse as Ecuador, India, South Africa and Brazil. These recent innovations have helped to expand the level and quality of coverage well beyond that achieved by traditional safety

net programmes; and they have increasingly attracted academic and political interest all over the world.

An important feature of social protection in developing countries has been the widespread implementation of programmes offering direct assistance to households in poverty. Among them, programmes providing cash transfers to households contingent on certain behaviour or performance (like school attendance and healthcare utilization) have been certainly the most popular. A common presumption, although without the sufficient empirical or theoretical evidence, is that CCT programmes are a more effective way to break the intergenerational transmission of poverty by encouraging poor households to invest in the human capital of their children (World Bank, 2009).

Research Aims and Contributions

The main objective of this project is precisely to look critically at the issue of *conditionality* in antipoverty programmes. More specifically, I intend to provide useful empirical evidence on this topic, in order to make a significant contribution to the ongoing debate about the role played by conditions in social protection initiatives. The persistent use of conditions in social assistance programs raises several questions regarding their true value and convenience that need to be answered to improve our understanding of poverty and its determinants. As a consequence, the role of conditions has attracted a great deal of attention from researchers and policy makers. In this thesis, I try to make a comprehensive assessment on the issue of conditionality in cash transfer programmes, which I consider important for the future development of improved forms of social assistance in developing countries.

I pursue this objective mainly by conducting impact evaluations that compare the effectiveness (in terms of welfare generation, human capital formation and labour supply) of the two most important cash transfer programmes offered in Ecuador: one with a standard design based on conditions and a recently implemented programme that provides

lump sum payments without explicit conditionalities attached. These impact evaluations are based on three different *quasi-experimental* methods (i.e. natural experiment, instrumental variable and difference-in-differences) and programme-specific evaluation designs, which by themselves represent an important *methodological* contribution to an evaluative literature on social assistance programmes where experimental methods have prevailed. In fact, it is striking that the majority of evaluations of this type of programmes in developing countries are conducted using randomized control trials (RCTs), which – besides being expensive and difficult to implement– raise considerable ethical concerns because they imply arbitrarily denying access to the programmes to many people who need and are entitled to receive cash transfers.

A large number of experimental studies have examined the effectiveness of traditional CCT programmes. However, to the best of my knowledge, no comparisons have been made between the outcomes of a programme with the common basic structure (namely, the BDH) and a programme that provides an unconditional lump-sum payment empowering poor families not only with money, but also with trust (namely, the CDH), all this within the same country and in the same period of time. Therefore, this research attempts to make a significant *empirical* contribution to the existing political and academic debate on the actual effectiveness of both types of programmes and on the independent benefits that their simultaneous implementation could have for the poor populations. This debate has intensified in Ecuador in recent years mainly because both programmes have coexisted for a long time and the unconditional CDH programme has not had important impact evaluations so far, when it has been nearly a decade since its implementation.

The different theoretical perspectives on cash transfers as an effective means of social assistance to overcome poverty, as well as the theoretical arguments for both inclusion and exclusion of conditions in the design of these programmes, are reviewed in depth as part of this research project. This initial analysis is intended to frame the investigation, help in the interpretation of the results, and establish whether (and why) conditions are actually considered the "keystone" of progressive social policy and effective social protection. Moreover, I also descriptively analyse the most important

social assistance programmes offered in Ecuador, with special emphasis on the innovative features that the CDH programme presents (apart from setting aside conditionality), such as the lump-sum cash transfers and the technical support received by beneficiary households to start their own businesses. In order to draw the right conclusions, it is important to understand first the main characteristics and the mechanisms through which new forms of social assistance are thought to be facilitating the productive inclusion of the poor and their sustained escape from poverty.

Ultimately, by linking the empirical results to the theoretical framework, this investigation tries to find out if conditionality has been a necessary and/or a sufficient element to guarantee the success of cash transfer programmes in Ecuador, and if it could be effectively replaced by other elements of a much positive nature (such as mutual *trust* between the parties involved). The attempt to answer these questions constitutes the main *theoretical* contribution of this thesis to the growing literature on cash transfer programmes, and more specifically to the long-standing social policy discussion about the role of conditions in social assistance practices.

This study quantitatively examines whether there are significant improvements in the welfare level, human capital investment and labour supply of the poor populations in Ecuador due specifically to the social benefits received by beneficiaries of the traditional BDH programme, as well as by those who opted at some point for the non-conditional CDH initiative. The research enquiry is driven by the following general research questions: Do the BDH and CDH cash transfer programmes affect the levels of well-being, schooling and unemployment of those in poverty? Does conditionality make a difference in terms of the efficiency of the programmes? Is it effective to set aside conditionality and trust the poor with lump-sum cash transfers?

CHAPTER 1
The Rationale for Cash Transfers and Conditionality

1.1 Introduction

The great popularity that conditional cash transfer (CCT) programmes have achieved, especially in Latin American countries in the last twenty years, seems to indicate that the 'Solomonic decision' they have found in the political and academic spheres to put an end to the long-standing debate about the effectiveness of income transfer initiatives has been the implementation of programmes with attached conditionalities. It is believed that the presence of conditions guarantees in some way the correct decision-making of the beneficiaries and avoids the possibility of generating dependency among those in poverty (Fiszbein et *al*., 2009). However, far from reaching a consensus, this has led to another – even more intense– discussion about the necessity and relevance of the element of conditionality in social assistance practices.

There is a widespread perception that CCTs are "a magic bullet in development" and a more effective way of compelling poor households to invest in human capital

compared to unconditional cash transfers (UCTs) (The Economist, 2005; Adato & Hoddinott, 2007). The basic premise for the inclusion of education and health-related conditionalities is that they are necessary to achieve programmes' objectives in terms of human capital formation due to the assumed inability of the poor to invest in their children (Bastagli, 2008; Maluccio & Flores, 2004). Proponents of conditionality argue that pure income transfers or UCTs will make beneficiary households more able to access basic services; but the transfers attached to behavioural conditions will have significant additional effects (Barrientos, 2011). This is because parents' decisions regarding the human capital of their children (e.g. school enrolment) are not socially or privately optimal (especially in the case of "marginal" children –like girls, young children, or those with lower ability) (Akresh et *al*., 2013).

Another typical argument in favour of conditionality is that conditions may be included to facilitate political and public support for social assistance programmes in developing countries, acting as a legitimate response to the usual political constraints faced by policymakers (Barrientos, 2011). In other words, conditionality makes redistribution to the poor politically more palatable to the non-poor. Basically, conditions provide a signal to concerned taxpayers that transfers will be associated with improvements in the human capital (or labour supply) of beneficiaries. Therefore, according to many scholars and politicians, conditions are essential to legitimate cash transfer programmes (Adato & Hoddinott, 2007). This premise receives support from some empirical evidence about the politics of conditions in Brazil and Mexico (Lindert & Vincensini, 2008; Levy, 2006).

Although the arguments in favour of the inclusion of conditions are supported by a good amount of empirical evidence that demonstrates the positive results of CCT programmes (Rawlings & Rubio, 2005; Danvers, 2010); it is very difficult to verify that these outcomes are due specifically to the presence of conditions and that, if not for them, the results would be different. It could also be argued that conditionalities are redundant because the majority of those in poverty would have, for example, sent their children to school or healthcare centres even in the absence of conditions relating to education and

health. In fact, there is a significant amount of evidence showing that UCTs have also large positive impacts on human capital accumulation (De Carvalho, 2008).

Thus, concerns about the effectiveness of conditionality are due to the fact that most of the existing impact evaluations do not show the counterfactual and independent effects of conditions (i.e. income effects vs conditionality effects).[3] This is mainly because measuring the impact of the same cash transfer programme with and without conditions is extremely challenging in the absence of experimental data, given the evident ethical and technical difficulties involved in such an experiment (Barrientos, 2011). In addition, the costs associated with the implementation, verification and compliance of conditions are also an issue of strong concern, since these costs represent an additional burden on the usually limited national budgets to fight poverty and because many times the costs of compliance cannot be faced by households with limited resources (Myamba & Ulriksen, 2016). Consequently, further research is necessary in order to definitely conclude whether conditions provide any significant additional effects, so that it is worth implementing them considering the high costs to be incurred (i.e. determine whether conditions are both effective and cost-effective).

Drawing on a review of the literature, this chapter provides a conceptual and theoretical framework by addressing topics such as poverty, social protection, social insurance, social assistance and minimum labour standards in developing countries. First, the definition, characteristics and types of cash transfer programmes are presented. Then, this chapter reviews the different theoretical perspectives on cash transfers as effective means to reduce poverty in developing countries. Finally, the current social policy debates on the effectiveness of cash transfers initiatives and the actual need to include an element of conditionality are discussed in depth. These themes together constitute the theoretical basis for the further development of this thesis.

[3] Two important exceptions are Todd & Wolpin (2003) and Bourguignon, Ferreira & Leite (2003) who used data on the Mexican Progresa and the Brazilian Bolsa Escola programs to simulate the impact of conditional and unconditional cash transfers.

1.2 Social Protection: Concept and Practice

The Organization for Economic Cooperation and Development (OECD) has developed a description of social protection as referring to "policies and actions which enhance the capacity of poor and vulnerable people to escape from poverty and better manage risks and shocks" (OECD, 2009, page 2). Social protection has also been associated in the work of the Institute of Development Studies (IDS) with all private and public initiatives that "provide income or consumption transfers to those in poverty, protect the vulnerable against livelihood risks, and improve the social status and rights of the marginalized" (Devereux & Sabates-Wheeler, 2004, page 9). In general, basic social protection refers to a variety of public institutions, benefits and programmes that protect individuals and their households from contingencies threatening well-being standards, independent of whether they have made an initial payment or belong to a specific community within the country (Barrientos, 2010).

Social protection policies encompass different types of measures that could be grouped into three main categories: social insurance, social assistance and minimum labour standards.[4] Social insurance consists of programmes that help individuals to smooth variations in consumption and income over time by contributing to a pool or fund, which is then used to pay benefits in the event of life-course or work-related contingencies such as old age, maternity, injury/illness, natural disasters, death (funeral insurance), or unemployment (Norton et *al*., 2001; Barrientos, 2010). Like all forms of insurance, it is a mechanism for pooling social and financial risks among individuals. Social insurance has represented one of the most important instruments through which social protection systems operate; however, it plays a more significant role for people in developed countries since poor households in less-developed nations, with limited and variable incomes, cannot always afford to make regular insurance payments (DFID, 2011). Although existing social insurance systems in developed countries have achieved

[4] Social protection might also include the provision of basic social services, such as health care and education.

reasonably high population coverage, this has not been the case in developing countries commonly characterized by informal economies and high rates of formal unemployment.

Labour and employment standards involve regulations and laws to ensure a decent working environment by setting and enforcing norms for minimum acceptable conditions at work, as well as imposing extended rights to organization and voice of employees (ILO, 2009; DFID, 2011). They range from fundamental rights (i.e. freedom from forced labour and child labour) to more specific rights, such as health and safety, maternity leave and minimum wages. These kinds of institutions have been fully established in developed countries after centuries of efforts and struggles. On the other hand, the development of adequate minimum labour standards in developing countries has been uneven because of different economic and political issues and controversies, such as affordability of providing a minimum wage and capability of ensuring its enforcement in vast informal sectors (Singh & Zammit, 2004).

The last, but not least, category of social protection is social assistance, which is generally described as support for those in poverty and extreme poverty. This support comes in the form of direct, regular and predictable social transfers to eligible individuals and households. Social assistance has a strong focus on poverty reduction, and it is the component of social protection where change, innovation and expansion have been more visible at least during the last 20 years in developing countries (Barrientos, 2010). A key feature of social assistance programmes is that they are usually fully financed with taxes, which means that -unlike social insurance- they are non-contributory (i.e. not financed from contributions by workers).

1.3 Social Assistance in Developing Countries

Barrientos (2010) argues in *Social Protection and Poverty* that social assistance plays a more important role within social protection strategies in developing countries, compared to its secondary role in developed nations. Social assistance in the developed world is

mostly set up as a safety net aimed to ensure minimum consumption levels to a small portion of individuals, but only when all the other (already well-developed) components of social protection (namely, social insurance and labour market regulations) have failed (Gough et *al*., 1997). The circumstances are completely different in low- and middle-income nations, where typically social insurance is insufficient, labour market regulations are weak, and access to basic services is highly stratified. Different factors (such as the extensive informality in the labour market) have complicated the development of social insurance as a poverty prevention measure in the Global South. Therefore, social assistance is called to become the main component of social protection and the key instrument to fight poverty and vulnerability in these countries.

Since the beginning of the twenty-first century, significant changes in priority given to different components of social protection in developing countries can be seen. These changes range from simple reforms of social insurance systems to the rapid introduction/expansion of innovative social assistance programmes (Barrientos, 2013). Most countries in the less developed regions of the world have introduced large-scale programmes of direct income transfers with the intention of reducing poverty and vulnerability. The rapid expansion of innovative social welfare programmes that incorporated income transfers to households and individuals, along with the significant reduction in global poverty over the last decade, has called special attention to what can be achieved by transferring resources directly to those in poverty.[5]

Generally speaking, direct income transfers may take the form of cash transfers, in-kind transfers (e.g. vouchers, food for work, school meals, etc.), or free access to goods and services (e.g. exemptions from education or health user fees). These transfers are targeted to a specific group of people based on their poverty situation measured through the income approach, multidimensional poverty indices or some other criteria. Receipt

[5] Global poverty and extreme poverty have declined significantly over the last few decades. According to recent poverty assessments completed at the World Bank, the number of people living under the extreme poverty line of $1.25 per day has halved since 1990, reaching around 1 billion people in 2011 (World Bank, 2014).

may be unconditional (i.e. pure income transfers) or conditional on certain behaviours (such as school or clinic attendance) or school/labour performance (for instance, based on school test scores). This research project focuses exclusively on cash transfer programmes (both conditional and unconditional), one of the most studied forms of development intervention, which, it is argued, have the potential to strengthen household productivity and their capacity for income generation (Arnold et *al*., 2011).

In the last decade, a new generation of integrated social assistance programmes in low- and middle-income countries –such as Uganda's Youth Opportunities, Chile's Ingreso Ético (Ethical Family Income) and Ecuador's Crédito de Desarrollo Humano– have adopted an alternative approach to facilitate the sustained escape from poverty through the *productive inclusion* of the poor, which is defined as a household's permanent engagement with the economy. On the basis that this engagement allows for a steady return that permits households to escape poverty in a sustainable way. According to Mariotti, Ulrichs and Harman (2016), these programmes represent a substantial change in social protection practices and they are based on the acknowledgement that merely investing in human capital –through traditional education and health-related conditions attached to cash transfers– may not help poor households to sustainably overcome poverty.

Nevertheless, the authors note that achieving productive inclusion requires first an "enabling economic environment" that offers sufficient and reachable opportunities for those in poverty, and this is sought through the combination of different public-sector policies or interventions. Overall, the results obtained by these new types of social assistance programmes have been positive, opening new paths in the fight against poverty and vulnerability in the developing world (Mariotti et *al*., 2016). The question then arises: what are the main design characteristics of these programmes that seek to help poor people through facilitating their productive inclusion?

The first and most basic feature of these programmes is that they provide *cash* (or asset) *transfers*, which can be regular or lump-sum (i.e. accumulated). Regular (smaller)

cash transfers are received over a longer period of time, while lump-sum (bigger) transfers are one-off payments that allow beneficiaries to use the money in something more productive in the long term than simply satisfying their immediate consumption needs. The periodic and cumulative delivery of cash transfers, in some cases specifically designed for investment in productive activities, is a way of turning them definitively into (non-reimbursable) productive micro-credits and, thus, give those in poverty access to financial products/services that have long been denied by the restrictive policies of private banks in the developing world.

However, productive inclusion is not achieved simply by facilitating the access of the poor to financial products. In other words, it may not be just about giving money to the people living in poverty. The poor have other important unmet basic needs (or deprivations) apart from access to credit, such as access to healthcare, education and technology. They are not on 'equal terms' with the rest of the population (mainly due to ignorance, ill health, lack of social services and shortage of financial resources) and, therefore, those in poverty do not have the same opportunities to develop their potential capabilities to the full extent (Sen, 2001). According to Sen's capability approach, poverty is merely understood as the deprivation or shortfall of basic capabilities. It seems at least difficult to change this common structural reality with just a simple cash transfer, no matter how big it is.

Therefore, a true productive and social inclusion requires to cover the unmet needs of the poor and offer opportunities with complementary practices that provide them with the necessary tools so that they can be self-sufficient, making the most of their capacities and resources (Sen, 2001; Mariotti et *al.*, 2016). Recent innovations in social assistance may combine the cash transfers with parallel and customized policies aimed at improving household productivity and entrepreneurship, such as asset and skills transfers, connections to income-generating activities and financial services. For instance, the Youth Opportunities programme in Uganda combines a one-off enterprise grant with vocational training. The most appropriate combination of interventions depends of course on the

context: the local economy, the target population, the labour market and institutional capacity.

It seems that it has finally been understood that in order to effectively reach and help the poorest and most vulnerable social groups, a personalized approach is required. For example, poor individuals with labour constraints (i.e. the elderly or disabled) will need a specific type of assistance for a longer (or even permanent) period of time, while the unemployed poor may simply need access to financial products and services or a 'leg up' to boost their participation in productive and better paid economic activities. In addition, for the first time, special emphasis is given to gender inequality issues through the design and implementation of gender-sensitive programmes that address the long-standing barriers women face in sexist societies when trying to get involved in productive activities and labour markets (Mariotti et *al*., 2016).

By linking income transfers in cash (or in-kind) with interventions that seek to break down the structural barriers that socially excluded people commonly face (instead of with conditionalities focused on improving their human capital), social assistance can be a powerful driver of sustained poverty escapes. Therefore, this new generation of integrated programmes is located within broader development strategies and stronger social protection systems, where typical social assistance practices are always linked to and complemented by the provision of strategic services (e.g. education, health, technical assistance, financial services). These social services must, of course, be specifically designed to meet the needs of those in poverty so that they can be more productive and reduce the risks inherent in their means of subsistence.

Even the main advocates of traditional CCT programmes, which have always placed greater emphasis on the role of conditionalities, have at some point recognized the importance of complementing cash transfers with public sector policies or practices. They have acknowledged that just giving money to the people living in poverty (even with conditions attached) is not the solution to all their problems (World Bank, 2009). Fiszbein, Schady and Ferreira (2009) note that while cash transfers are an effective way to

redistribute income among the poor, even the best designed and managed programme may not meet all the needs of an integrated system of social protection. For instance, the fact that children attend school more (due to conditionalities) does not necessarily imply that they learn more. In order to improve learning, CCTs should preferably be accompanied/complemented by better quality education and health services that provide children with a real advantage, be it through better nutrition or school programmes (Fiszbein et *al*., 2009). The problem has been that due to the very structure of CCT programmes, any complementary service is seen as an additional intervention that requires additional efforts (political and economic), so these practices have not been frequently carried out in developing countries.

Finally, a frequent (but not always present) feature of the new generation of integrated social assistance programmes is *trust* in the poor. The government's confidence in the capacities of its citizens is expressed through cash (or asset) transfers without attached conditionalities, which provide those in poverty with the possibility of doing with the transfer what they consider most appropriate for them.[6] The delivery of unconditional transfers is aligned with the rationalist perspective (based on rational choice theory) and combines in some way what could be considered an altruistic practice with the doctrine of economic liberalism. The idea behind this approach is that the best way to end poverty is simply by giving money to the poor since they can make their own decisions much better than a group of bureaucrats (Anderssen, 2010). On the other hand, this goes against the paternalistic perspective, according to which it must be controlled what is done with taxpayers' money and the government should at least try to influence poor households so that they adopt certain behaviours commonly related to better lifestyles.[7]

[6] Note that the opposite is not always true: the absence of conditionality does not necessarily imply confidence on the part of the government, it could simply be lack of budget.

[7] The rationalist and paternalistic perspectives on cash transfers will be analysed in depth later in this chapter.

In recent times, research on trust has become an area of great interest and importance in business management, behavioural economics and the social sciences in general. Nevertheless, providing a universal definition of trust remains a very complicated task, which is why much of the academic literature on trust focuses on definitional issues and philosophical debates. In any case, academics seem to agree on certain fundamental issues related to trust: it is not simply a behaviour (e.g. collaboration) or a choice (e.g. taking a risk) that involves perceived probabilities, or a set of positive expectations and beliefs (Bhattarcharya et *al*. 1998). Rather, trust is "the willingness to be vulnerable under conditions of risk and interdependence" (Bachmann & Zaheer, 2006, p. 307); it is an underlying psychological condition or state of mind "comprising the intention to accept vulnerability based upon positive expectations of the intentions or behaviour of another" (Rousseau et *al*., 1998, 395). Trust implies an affective attitude (Jones, 1996), an emotion, even a passion (Flores & Solomon, 1997).

Bachmann and Zaheer (2006) claim in *The Handbook of Trust Research* that trust permeates all levels of society. Indeed, individuals can trust one another. They may also trust an institution, as when parents trust a school to provide competent education to their children. And individuals can trust society, as when people trust society to assist them when a natural disaster strikes. Similarly, institutions may trust individuals, other institutions, and society in general. Finally, society may trust citizens, public and private institutions, and even other societies. However, building a relationship of trust is not easy to achieve, since it depends on several factors, actors and circumstances. Bachmann and Zaheer (2006) list the conditions they consider necessary to establish a trusting relationship:

- ✓ Interdependence: at least one of the parties in a trust relationship must depend on the other party for the achievement of an objective.
- ✓ Vulnerability: at least one of the parties in a trust relationship is vulnerable to the opportunistic behaviour of the other party.
- ✓ Risk: as a result of this vulnerability, the interests of at least one of the parties in the relationship are at risk.

Note, therefore, that there is an ethical component in any trust relationship, which arises from the vulnerability condition of the trusting party in the relationship to the goodwill of the other party. A trustworthy party in a relationship is one that will not *unfairly* exploit the other party's vulnerabilities. According to the authors, "ethics enters the picture when we need to decide what accounts as an unjust or unfair exploitation of the vulnerability of one of the parties in the trust relationship" (Bachmann & Zaheer, 2006).

Hence, the arbitrary choice to eliminate certain rules (or conditions) resulting from a rational calculation or based on experience, does not necessarily imply the subsequent generation of trust. In terms of social protection, for example, it could be the case that an unconditional programme is implemented, not because of the conviction and willingness of the policy-makers, but because of the lack of resources to control and monitor compliance with the conditions. When this occurs, it is easy for programme beneficiaries to perceive the absence of trust in the decision and act accordingly (as could be also explained by game theory models). On the contrary, when the decision to remove conditionalities is clearly voluntary and based on positive expectations, it is probably much easier to build a trusting relationship between programme implementers and beneficiaries.

By trusting the poor and their capabilities what is sought is to accept conditions of vulnerability and interdependence to make them aware that, ultimately, they are the only ones capable of changing their situation of poverty and vulnerability; and also signal them that, even when there are positive expectations about their intentions, they are the only ones responsible for decision-making that affects their quality of life. In other words, what is sought is to treat those in poverty as capable and responsible adults.

Evidently, the new generation of integrated social assistance initiatives have not yet reached their full potential as mechanisms to sustainably escape from poverty. Along the way, they have encountered different constraints, such as tight budgets, the scarce availability of resources (economic and human), the lack of political will and, quite often,

the lack of support from taxpayers who do not welcome alternative (and unconditional) cash transfer initiatives. Therefore, social assistance programmes of this kind must continue to improve and, for this to happen, it is important to evaluate their results on poverty reduction. Empirical evidence is necessary, for example, to be able to justify the argument that the option of productive inclusion is more effective than traditional conditions. Only through scientific research, is it possible to design more and better integrated social assistance programmes that can meet the different needs of the poor and, subsequently, help more people to escape from poverty in a sustainable and profitable way.

1.4 Cash Transfer Programmes: Definition, Characteristics and Arguments in Favour and Against

As mentioned above, cash transfers are a specific form of social assistance consisting of direct, regular and reliable non-contributory payments with the aim of reducing poverty and vulnerability (Arnold et *al*., 2011). Cash transfer programmes could be unconditional (i.e. without conditions or strings attached) or conditional on a certain action, performance or behaviour (such as regularly accessing maternal and child health care services and/or sending children to school) (Doetinchem et *al*., 2008; Arnold et *al*., 2011). The beneficiaries of these programmes are typically the poorest households in a country and also those sectors of the population considered vulnerable, such as older people, disabled and orphans (Cecchini & Madariaga, 2011).

The fundamental assumptions behind these popular practices are that those in poverty can be *empowered* and *entrusted* to make effective/productive use of the monetary resources available and that, despite the multidimensional condition of poverty, low and variable income is a key element of this problem (Lister, 2004; Arnold et *al*., 2011). The money inflows are intended to raise and smooth households' incomes (and consumption) in the short-term and to facilitate human capital accumulation in the medium- and long-

term (Bourguignon et *al*., 2003). Through investments in health, education and nutrition, it is thought that poor households can break the intergenerational transmission of poverty.

There are some arguments both for and against giving money directly to the poor. Cash transfer programmes are generally criticized on the basis that people make irrational or uninformed decisions, and therefore they should not be entrusted with public resources (Banerjee and Duflo, 2012). In other words, according to this claim, the poor should not be trusted with cash transfers because they normally make incorrect life decisions. Moreover, some argue that the implementation and development of these programmes is expensive and not sustainable in the long term (McCord, 2009). Finally, once money becomes a determining factor in a relationship, this could negatively change its power dynamics. The relationship between the beneficiaries and the state could be affected if individuals become, to some extent, dependent on the economic aid that they receive from the government (Blank, 2003).

On the other hand, there are also strong arguments in favour of the implementation of cash transfer initiatives. It is claimed that this form of social assistance allows households to build human capital and accumulate assets over time, without the need to take on debt (Barrientos, 2010). The formation of human capabilities is considered a key factor for sustainable development and progress in every society (OECD, 2001). Substantial evidence suggests that higher levels of education, training and other forms of human capital have positive and significant effects on individuals' earnings and, therefore, they can be seen as reasonable ways to escape poverty (Barro, Caselli, & Lee, 2013). In addition, cash transfers may also help to overcome cost barriers (e.g. transportation costs) that restrict access to essential public services. Thus, these programmes might weaken social exclusion, allowing more participation of those in poverty in their communities and societies (Arnold et *al*., 2011). In general, these arguments can be summarized in the various ways in which money inflows can help the poor (i.e. protecting household consumption, strengthening productive capacity and promoting asset accumulation).

Most of the impact evaluations carried out have been in favour of the efficacy of cash transfer programmes (Fiszbein et *al*., 2009; Arnold et *al*., 2011; Robertson et *al.*, 2013). The empirical evidence suggests that cash transfers are successful facilitators of investments in human capital (Adato & Hoddinott, 2011). As a result, and despite the theoretical debate about their real effectiveness, these programmes have been extensively adopted in the last two decades as innovative forms of social assistance for those in poverty – especially the type of programmes that provide money to households contingent on certain behaviours (i.e. CCTs). In fact, as of 1997, most Latin American countries have implemented and evaluated different CCT programmes with health and education components. Subsequently, and in the light of the outcomes obtained, many developing countries in other regions of the world also introduced cash transfer programmes with similar characteristics (Cecchini & Madariaga, 2011; Khan et *al*., 2016).

Traditional CCT programmes (i.e. those with human development conditions) have been considered for years, by policymakers and important non-governmental organizations (like the World Bank), as the most effective social assistance instruments to fight poverty by better tackling the underinvestment in human capital (Fiszbein et *al*., 2009; World Bank, 2009). The stated reason for such consideration –apart from their novel and attractive design– is the common belief/theory that poor households lack full information on the benefits of healthcare and education (Arnold et *al*., 2011). Therefore, it is thought that the use of conditions is necessary to commit them to send their children to school and/or bring them to health centres for regular check-ups, prenatal care and to receive infant nutritional supplementation (for example, minimum school attendance rates of 85 percent are required in Brazil's Bolsa Familia and a similar restriction applies in Mexico's Oportunidades).

Note that CCTs represent an attempt to steer a middle way between arguments in favour and against the implementation of cash transfer programmes in developing countries. Interestingly, the inclusion of the element of *conditionality* allowed reaching a kind of consensus on the heated debates pro- and contra- giving money to those in poverty. The notion behind CCTs is that even if the basic assumption behind giving money to the

poor (i.e. that people make effective use of available monetary resources) is not satisfied, cash transfer programmes are still attractive if they are attached to a certain bundle of conditions that encourage individuals to pursue "desirable" actions (Cecchini & Madariaga, 2011).

1.5 Different Theoretical Perspectives on Cash Transfers

1.5.1 Rational Choice Theory

The idea of using monetary transfers to smooth consumption, increase demand for public services, and ultimately to encourage investment in human capital, has its roots in a well-known premise of traditional economic theory: the notion that individuals always make logical and prudent choices that provide them with the greatest benefit or satisfaction and that are in their highest self-interest (Bassett, 2008). In other words, the economic principle of *rational choice* is clearly the core theory underpinning the most basic type of cash transfer programmes with no strings attached (i.e. UCTs) (Arnold et *al*., 2011).

Rational choice theory –also known as rational action theory or just choice theory– is the most common theoretical framework for understanding and formally modelling economic and social behaviour (Blume & Easley, 2008). It is generally used to describe the way individuals think in conventional economic theory and in most microeconomic models, where it is interpreted as "wanting more rather than less of a good". It is noteworthy that the notion of *rationality* described by choice theory is different from both the conventional and the more philosophical usages of the word. While in colloquial language rationality means "sane" or "in a lucid and reflective manner", choice theory uses a more precise and narrow definition of rationality to mean that agents maximize their utility (i.e. happiness) taking into consideration all costs and benefits associated with

each decision they take (Friedman, 1953). In choice theory, to be rational means to be capable of assessing one's own best interests, even when others do not agree with those assessments.

If this rationality assumption holds, one may expect that all the money received through transfers will be employed in the most effective manner (i.e. the one that maximizes a household's utility function) because individuals' decisions are made on the basis that perceived benefits outweigh perceived costs. Additionally, when poor families receive monetary incentives, their cost-benefit considerations regarding important decisions are going to change, which, in turn, will affect their decision-making calculus. For instance, a cash transfer might reduce the opportunity cost of enrolling children in school (namely, the money they earn if they go to work instead), making the benefits of this decision compensate the costs (Bassett, 2008). Therefore, the arguments in favour of cash transfers as an effective social assistance policy are clearly supported on the basis of this theoretical perspective. In other words, rational choice theory clearly supports the implementation of the transfer programmes, and more specifically the unconditional ones since only the 'freedom of choice' guarantees that the monetary and human resources will be used in the most efficient way for the beneficiary.

Hanlon, Barrientos, and Hulme (2010) rely on the idea of rationality to argue in their book *Just Give Money to the Poor* that the biggest problem faced by people living in poverty is a basic lack of cash and not the lack of motivation or knowledge. Thus, simply transferring money to them –no conditions attached– may be the most favourable approach not just for reducing poverty, but for promoting long-term economic growth and human development. The authors are fully convinced that poor people should be *trusted* in their knowledge and ability to make decisions that promote their own well-being, and also that the government or the external donors will continue making incorrect/inefficient decisions on behalf of the poor.

This trust-based perspective on cash transfers can be explained using concepts, principles and theories from different social sciences, including of course rational choice

theory. In economics and sociology, for example, trust can be seen as an "economic and social lubricant" that reduces the costs of transactions and enables new forms of cooperation among individuals as well as societies (Morgan & Hunt, 1994). Moreover, from the perspective of game theory, trust can provide an explanation for moving from the Nash equilibrium to the socially optimum equilibrium. As a result, trust has been considered by many social scientists as an important form of *social capital* and its process of creation and distribution is a recurrent theme of research.

There is some empirical evidence in developing countries that supports the idea of trusting on the decision-making ability (or rationality) of the poor by showing that UCTs do not increase purchases of so-called "temptation goods" (such as alcohol, narcotics, and cigarettes), but rather this money is mainly spent on productive investment, education, and food. Popova and Evans (2014) conducted a comprehensive review of 19 quantitative studies on the impact of different types of cash transfers on consumption levels of temptation goods. Almost none of these studies concluded that expenditures on this type of goods increased with the implementation of social assistance programmes. On the contrary, the evidence points out clearly that there are significant negative effects in most of the cases (i.e. expenditures on temptation goods actually declined). Remarkably, no significant differences in the effects were found between programmes with attached conditions and programmes without them.

Another recent major review of the experience of cash transfers in low- and middle-income countries was performed by Blattman and Niehaus (2014). They took into account various studies on the economic behaviour of people receiving money through cash transfer programmes in countries as diverse as Uganda, Ghana and Sri Lanka. These studies consistently indicate that beneficiaries normally make good decisions about the use of transfers. More specifically, the evidence shows that in most cases the money that poor people receive is not used to buy luxury or temptation goods, but rather invested in productive activities and used to cover basic household needs, such as food, clothing, and healthcare.

Moreover, Devereux (2009) conducted two different controlled experiments in Malawi and Namibia and he found that unconditional basic-income grants positively affected children's school attendance and nutrition practices. Similarly, Ozler, Mcintosh and Baird (2010) found no difference between the effects of conditional and unconditional cash transfers on school attendance of girls in Malawi. This important study was one of the first to challenge the rationale for CCTs by suggesting that conditions are not the determining factor.

But perhaps one of the most striking studies that have been done in recent years on this subject is the one conducted by Haushofer and Shapiro (2013). Using a randomized controlled trial, they analyse the response of poor rural households in rural Kenya to large temporary income changes caused by unconditional cash transfers of at least 404 US dollars. They find that households receiving transfers experienced an increase of 58 percent in durables, and a reduction of 30 percent in the likelihood of having gone to bed hungry. Additionally, they do not find evidence of increased expenditure on temptation goods. Together, these results suggest that cash transfer programmes without conditions have significant impacts on consumption, food security, investment and psychological well-being of the poor in developing countries.

However, despite these efforts, there are still not sufficient research studies evaluating the effectiveness of UCTs, especially in middle-income Latin American countries with well-developed public services, perhaps because in most of these countries the CCT programmes have been the most widely accepted and popularized initiatives. Therefore, most scientific efforts aimed at assessing social assistance practices have focused on this other type of programme. Besides, there is also some evidence suggesting that UCT initiatives have had only a limited success (Fiszbein et *al*., 2009; Arnold et *al*., 2011). This has led academics and policymakers to doubt the potential of unconditional transfers to achieve the long-awaited eradication of poverty.

1.5.2 Welfare Trap Theory

The notion that people behave in a rational and self-interested manner can be, however, interpreted in a completely opposite way. In fact, the notion of rationality is also frequently used to support the idea that poverty is caused by the very efforts to alleviate it. This perspective, which is evidently against all forms of social assistance including cash transfers, was developed by George Gilder (1981) and it is usually referred to as *welfare dependency* or *welfare trap theory*. The premise of this neoclassical theory of economics is that welfare programmes cause poverty because, once they are implemented, a guaranteed income is created that can incentivise the poor to make the 'rational' decision to avoid work; while, at the same time, an equivalent tax is imposed on workers that can also create a disincentive to continue working (Blank, 2003; Jung & Smith, 2007).

In consequence, also using rational choice, it can be argued that most of the time those in poverty would prefer more welfare (and leisure) and less work (Fiszbein et *al*., 2009). Proponents of this hypothesis argue that recipients of cash transfers develop a "culture of dependency", which means that government's social assistance programmes supposedly undermine values, beliefs and attitudes (like the desire to find a job and escape poverty), since welfare recipients get used to the state providing for them. The main arguments against the implementation of cash transfer programmes are clearly based on this theoretical perspective, which has become a recurring theme of analysis and evaluation in a significant number of published papers on poverty and the welfare state.

One of the most classic and frequently cited articles is Kasarda and Ting's (1996) study of skills mismatch and spatial mismatch in post-war American cities. The authors argue that the poor are smart (or rational) people who are capable to seek public assistance and realize that a combination of social benefits will produce a higher salary than the one offered by the typical low-paid work positions. People living in poverty have access only to these jobs due to a combination of two phenomena: 1) lower-income residential districts are producing a surplus of high-skilled jobs that most urban welfare recipients cannot obtain because they do not have the appropriate skills; 2) low-skilled jobs have moved out

to the sub-urban locations, where recipients cannot afford to live. Therefore, reductions in structural poverty and welfare dependence would rely on significant changes in policies related not only to economic development and social assistance, but also to housing (e.g. decentralize affordable housing) and transportation (e.g. improve transit options).

It is noteworthy that, while there is some agreement about the nature of welfare dependency, there are still many doubts about the scope of the detrimental effects of welfare programmes on the willingness to work (i.e. the size of the disincentive) (Jung & Smith, 2007). Actually, there is no conclusive evidence of the causal link between social benefits and labour supply. A survey by Danziger, Haveman and Plotnick (1981) looks at the case of the former Aid to Families with Dependent Children (AFDC) programme in the United States that provided financial assistance to children whose families had low or no income. They estimated that the AFDC programme created an incentive to avoid work in at least 10 per cent of the welfare recipients. On the other hand, Moffitt (1992) conducted a complete review of all the literature related to the AFDC welfare programme from 1965-1985 and he found no strong evidence that benefits have a detrimental effect on labour force participation. Although most studies showed consistent evidence of negative correlation between welfare benefits and labour supply, the author argues that the results cannot be interpreted as causal.

Surprisingly, there is not much empirical evidence about the disincentive effects of welfare programmes on adult labour supply in developing countries. Most studies about the impact of cash transfers on adult work decisions have focused on high-income countries like the US, UK and Canada. However, an important exception is Bourguignon, Ferreira and Leite (2003), which uses ex-ante micro-simulation to analyse the income effect of the Bolsa Escola programme on adult labour supply in Brazil. Even though they find negative effects on labour supply, these are statistically and economically insignificant.

1.5.3 Paternalistic Theory

The two different perspectives about cash transfers discussed above, although contrary, are mainly based on the same economic principle (i.e., rational choice). There is another point of view opposed to the idea that poor people are capable of making rational decisions, which is supported by the notion of *paternalism* and the wider framework of behavioural economics.

Paternalism is defined as the unconsented interference of a state with a person or group of persons, motivated or under the argument that the interfered-with person(s) will be in a better position and protected from harm (Dworkin, 2014). In the context of social assistance, the premise of the paternalistic theory is that those in poverty are anything but rational when it comes to important choices about their lives. This could be due to an innate condition of human beings or because the poor do not have complete information on the long-term benefits of certain decisions (such as investment in education and health) (Arnold et *al*., 2011). In any case, according to the paternalistic view, poor households are unable to look out for themselves because they usually make decisions that negatively affect their own well-being, wealth or happiness. Therefore, people living in poverty cannot be trusted with money from tax-payers without authorities making some decisions for them, forcing them to comply with certain rules, and urging them to do the "right things". In other words, this theory suggests that policy-makers need to build a kind of "choice architecture" to help ensure that the poor choose correctly (Hanlon et *al*., 2010).

Note that this anti-neoclassical perspective takes into account some of the arguments for and against the delivery of cash transfers. On the one hand, it recognizes the importance of assisting those in poverty with public monetary resources and at least acknowledges in them some individual capacity to spend the money from transfers. On the other hand, this approach considers essential the role played by the government by guiding the budgetary expenditure of families and modifying their behaviour, so that they make optimal investment decisions in human capital and labour supply, while at the same

time reducing the possibility of generating dependency on these programmes. If so, what are the visible implications of the paternalistic perspective on social assistance policies?

Every rule, policy or action in a society may be enacted for various reasons and, therefore, may have different justifications. However, when a policy is justified merely stating that the affected people would be better off due to such intervention, and individuals are imposed upon regardless of their will, we have an instance of paternalism (Dworkin, 2014). This is exactly the case of *conditions* attached to cash transfers, which constitute a clear example of a commonly adopted paternalistic measure in antipoverty programmes. Therefore, paternalistic theory does not preclude the application of cash transfers as a tool for poverty alleviation, provided that these programmes are accompanied by a set of specific requirements usually related to "desirable" behaviours among the poor. In other words, this theoretical approach supports the implementation of monetary transfers, but only if they are linked to the fulfilment of certain conditions.

There has been a powerful trend in academic and policy-maker circles all over the world towards the acceptance of the paternalistic perspective with respect to welfare programmes. This tendency is clearly reflected in the widespread implementation of CCTs in many developing and developed nations (Rawlings & Rubio, 2005; Behrman et *al*., 2010). They are currently present in over 30 developing countries, primarily in Latin America, but also in Africa and Asia. According to the Economic Commission for Latin America and the Caribbean, eighteen out of twenty Latin American countries have at least one CCT programme currently operating, and they cover as many as 25 million families (about 129 million beneficiaries), which represents the 20 percent of the total population in the region (CEPAL, 2010).

An important reference of the paternalistic perspective is Fiszbein et *al*. (2009), who argue in their book *Conditional Cash Transfers: Reducing Present and Future Poverty* that those in poverty maintain incorrect ideas about the profitability of investing in the human capital of their children; and also, they emphasize the positive outcomes of CCT programmes in developing countries. This famous book –called by IRIN (2010) as

the World Bank's *Bible* on CCTs– provides a detailed summary of existing programmes and the most important impact evaluations, as well as a thorough economic justification for investing in programmes with attached conditions over other social investments on the supply side. Thus, although the authors acknowledge that there is limited evidence on the feature of CCTs that matters most (either cash transfers or conditions), they are fully convinced that conditionality is a key policy mechanism to help the poor make better decisions and that conditions play a very important role in the design of more efficient programmes.

A growing number of recent studies have assessed the effectiveness of most of the CCT programmes in Latin America through rigorous impact evaluations. They consistently find significant positive effects of CCTs on income poverty reduction, health and education (measured through school enrolment and educational attainment) (Rawlings & Rubio, 2005; Danvers, 2010). Moreover, they also show that increases in the level of instruction of children are usually accompanied by decreasing child labour supply. In Ecuador, for example, school enrolment of children whose families are around the first quintile of the country's poverty index increased by about 10 percentage points in response to the Bono de Desarrollo Humano (BDH) conditional programme (Schady & Araujo, 2006). Besides, paid and unpaid labour supply declined by around 15 percentage points among youths (Edmonds & Schady, 2012). Finally, benefits from CCT programmes have been reported in gender equality (Soares & Silva, 2010). Overall, the empirical evidence suggests that the adoption of paternalistic measures regarding social assistance has been successful and has had positive and promising results in many developing countries.

1.5.4 Behavioural Intervention Approach

As one can realize from the previously explained perspectives about social assistance programmes, monetary *incentives* might play an important role in modifying human

behaviour. Though, the scope and magnitude of the effects that incentives may have on behaviour depend on various economic, social, political, and psychological factors. This gives rise to a completely different approach to understand and ultimately justify the implementation of cash transfers, which is based on the emerging literature about incentives in *behavioural interventions*.

The use of (monetary and nonmonetary) incentives to foster desired behaviours among individuals has been a topic of relatively recent analysis by different social sciences and, as a result, it has provoked an interesting debate about the effectiveness and *ethics* associated with these practices. The advocates of using incentives in behavioural interventions support their arguments in the so-called "law of behaviour", which states that higher incentives will lead to more effort and consequently higher performance. On the other hand, those opposed to such practices argue that using incentives in sensitive areas (like education, health, etc.) could be counterproductive, given that external incentives may displace internal (or personal) motivations that are essential to achieve individually and socially desirable behaviours (Gneezy, 2011). This phenomenon is known as the *crowding-out effects* of monetary incentives.

The scientific interest on the mechanisms through which monetary incentives may increase or reduce effort and motivation to undertake a task (i.e. price effects versus crowding-out effects) has been present in the fields of psychology and economics since the early 1970s (Gneezy, 2011). Some important conclusions may be drawn from the empirical research on this topic. First, it seems to be that incentives contain information (or signals), which are transmitted indirectly from the principal to the agents, triggering unexpected effects on behaviour once the agents draw conclusions from the existence, sustainability and the size of the incentives (Frey & Oberholzer-Gee, 1997).[8] Second, explicit incentives may break well-established social norms and cause large crowding-out effects (Titmuss, 1970). For example, monetary incentives do not necessarily increase

[8] Frey and Oberholzer-Gee (1997) show that by offering unexpectedly large cash compensations to the members of a community for the presence of a nuclear plant, the principal signals to the agents that the risks are high, and in consequence people may be even less willing to accept the presence of the plant.

voluntary contributions to public goods (such as donating blood or protecting the environment); in fact, they may have the opposite effect. Finally, if the size of the incentive is large enough, the standard price effect will surpass the crowding-out effect (at least for some time), and therefore, the incentivized behaviour will be more attractive for the agent. However, if the incentives are too large, the opposite may happen because people can "choke under pressure" (Ariely et *al*., 2009).

In the context of CCT programmes, which are basically monetary incentives designed to encourage human capital investment among those in poverty, a potential conflict arises between the direct extrinsic effect of the social benefits and the previously explained crowding-out effects. On the one hand, extrinsic incentives provide immediate returns that aim to compensate the usually high opportunity and transportation costs of sending children to school. Additionally, the money delivered to the parents may give children an extra motivation to study. On the other hand, monetary incentives may crowd out important intrinsic motivations to go to school and invest in children's education. In other words, cash transfers may displace more important underlying reasons for pursuing education, such as improving skills, expanding the knowledge base, obtaining better career opportunities, successfully entering the labour market and personal satisfaction. Therefore, the effect of cash transfers will depend on the way agents perceive the incentives, the characteristics of the programme, and the size and frequency of the benefits.

A number of recent studies have assessed the use of cash transfers in behavioural interventions using field experiments in schools. Angrist, Bettinger, and Kremer (2006) look at two programmes in Colombia (Familias en Acción and PACES) that randomly assigned vouchers partially covering the costs of secondary school in exchange for school attendance and academic progress. The authors find that voucher-holders were about 10 percent more likely to finish the school year obtaining slightly better scores on achievement tests. Moreover, Bettinger (2010) studies behavioural responses to monetary incentives in primary schools in Ohio (US), where a private foundation sponsored cash transfers of 100 US dollars for improvements in academic performance (measured through

test scores). Using a stratified randomization method, he finds that direct incentives for higher grades increased scores in concrete subjects, such as math; but did not improved academic performance in more conceptual subjects, like social sciences or reading.

Overall, most of the existing empirical evidence about the effectiveness of monetary incentives in modifying human behaviour shows that: 1) cash transfers have positive effects on different education-related outcomes, like school attendance or academic performance; and 2) the size of the effects depends on the programme's design and how the extrinsic incentive interacts with intrinsic and social motivations (Gneezy, 2011). Therefore, according to the behavioural intervention approach, conditional (or incentive-based) cash transfer programmes (when properly designed) have the potential to be effective in modifying the behaviour of people living in poverty. However, there are certain values considerations and ethical concerns about this type of practices that could undermine the effects. In addition to going against principles of freedom and equality, the use of conditions (or incentives) could be counterproductive if, for example, the individual loses sight of the fundamental reasons for educating their children or seeking work, such as the spirit of personal growth and the value of knowledge.

In conclusion, the different theoretical perspectives about cash transfers reviewed in this section raise a number of interesting issues relating to our understanding of human behaviour, incentives, the role of the government, the importance of social assistance and poverty in general. The apparently simple concept of cash transfers overlies a great deal of complexity. The difficulty of addressing social assistance comes from multiple reasons, such as the multidimensional nature of poverty, the complexity of human behaviour and the ethical considerations that emerge from these practices. Therefore, social assistance policies should be studied from different perspectives and a multidisciplinary approach becomes a necessity for this purpose. Although the debate on whether poor households should be given cash transfers (with or without conditions) as a social safety net to alleviate poverty has been around for decades, existing theoretical frameworks and empirical evidence provide some important insights about when and why cash transfers are more likely to succeed in developing countries.

1.6 The Element of Conditionality

As discussed in the previous section, the different perspectives on cash transfers also offer distinct approaches for the inclusion of an element of conditionality in social assistance programmes. However, as noted by Clasen and Clegg (2007), welfare programmes are always conditioned to a certain extent since they require some input from potential beneficiaries (e.g. citizenship, residency or registration documentation). For that reason, the authors develop a framework to analysing conditionality in social policy that distinguishes between three possible levels of adjustment that conditions have for the provision of social benefits: conditions of category, conditions of circumstance and conditions of conduct. In this thesis, the main focus is on the conditions related to the conduct of the beneficiary households, specifically the behavioural requirements that have to do with their human development (i.e. education and health). Therefore, the following sub-sections will refer to this third and final level of conditionality that has stood out in recent welfare state discourse (Clasen & Clegg, 2007). Some important aspects of work-related conditions/eligibility criteria (i.e. second-level circumstance conditions) are briefly reviewed in the last sub-section.

1.6.1 The "Default Position" for Cash Transfers: Well-Functioning Markets

Even the strongest advocates of conditionality recognize that if transfer beneficiaries are well-informed and rational actors, markets are well-functioning, and governments are benevolent to their citizens, the "theoretical default position" for cash transfers should be in favour of payments with no strings attached (Fiszbein et *al.*, 2009). In order to understand this assertion, it is necessary to comprehend each of the listed conditions under which UCTs are considered the ideal form of social assistance. The theoretical framework

behind agent's rationality (i.e. choice theory) was already analysed in the previous section (see page 24). It is now the turn of the *well-functioning* markets condition.

In neoclassical economics, a perfect market is one in which competition reaches its highest possible level and thus produces the best results for consumers and society. According to the theory of perfect markets, the set of collective decisions of individuals – who behave like rational actors trying to maximize their own well-being– is the key element to create "the invisible hand" of the market. Figuratively speaking, this invisible hand distributes goods and services in the most efficient manner (Mas-Colell, 1995). The structure of a perfect market is known as pure or perfect competition. This type of competition is characterized by the fulfilment of some well-known theoretical conditions that include the following: non-intervention by governments; perfect market information; no barriers to entry or exit; no participants with market power to set prices; profit maximization; equal access to factors of production; and no externalities (Mankiw, 2012).

Evidently, markets in the real world are never perfect and the conditions for perfect competition are not possible to fulfil in practice. However, some basic structural characteristics can be approximated for real world markets to be considered at least "well-functioning", which is the closest thing to perfect markets: a large number of consumers and producers with the willingness and ability to buy/sell products at a certain price; all agents have equal access to relevant information with which to make a decision (i.e. perfect information); and the costs or benefits of an activity do not affect third parties (i.e. no externalities).

Therefore, if individuals behave rationally, the government does not make decisions based on its own benefit, and the mentioned conditions for well-functioning markets are satisfied, the best way to assist those in poverty must be with *unconditional* cash transfers. The intuition behind this argument is that –under such circumstances– for any given amount of aid, an individual can achieve either the same or higher utility with an unconditional cash transfer than when the same transfer is given in-kind with a no-resale condition or when the same budget is used to subsidize a specific good or service.

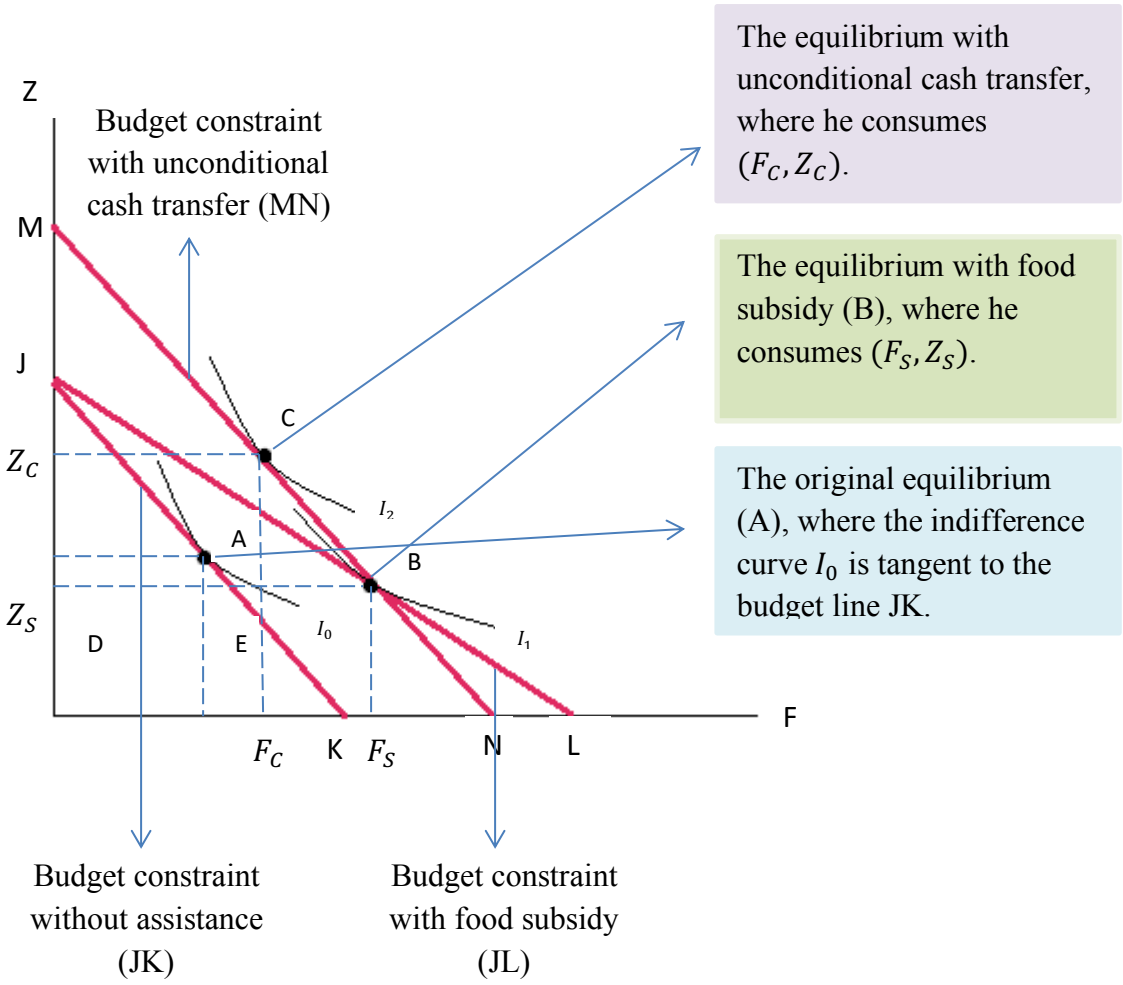
Note that a cash transfer that is conditional on the purchase of a good or the use of a service (e.g. education or healthcare) is equivalent to such a subsidy (Tresch, 2008; Fiszbein et *al*., 2009).

The Revealed Preferences Model

At this point it seems important to theoretically prove that –under certain strong conditions– unconditional programmes are the most efficient form of social assistance and, therefore, they should be the default position of cash transfers. This can be done by what economists refer to as a standard *revealed preferences* model. The underlying assumptions of the model are, of course, that individuals behave rationally and that markets meet the conditions to be well-functioning. Suppose there are many rich people (R), and one poor person (P). There are just two goods, food, F, and one other commodity that serves all other purposes, Z. P receives utility from his own consumption of the two goods: $U^P = U^P(Z_P, F_P)$. (See Figure 1)

Suppose the rich worry about P's utility and they think that he has inadequate amounts of necessary goods and services, such as food, education, medical care, housing, etc. They conclude that his problem is merely that he lacks the income necessary to have an adequate standard of living. Since the rich care about P's well-being, their utility function is $U^R = U^R(Z_R, F_R, U^P(Z_P, F_P))$. I_0, I_1, and I_2 are three of P's indifference curves for Z and F. His budget constraint without any assistance is represented by JK, and its slope equals the relative price of the two goods. The original equilibrium is at point A, where the indifference curve I_0 is tangent to the budget line. Figure 1 illustrates this graphically from the point of view of beneficiary P.

Figure 1: Revealed Preferences Model



A food subsidy (or an in-kind transfer of food) only rotates the budget line to JL. The new equilibrium is at point B, where the indifference curve I_1 is tangent to the new budget constraint. The beneficiary of the subsidy consumes (F_S, Z_S). He spends DE on food out of his own resources and receives a subsidy of EB from the government.

Now, suppose that instead of the subsidy he receives an unconditional cash transfer of GH. The increased income shifts the budget constraint out parallel to JK. The new budget line is MN and the equilibrium is at point C, where he consumes (F_C, Z_C). As

clearly seen in the figure, he consumes more food with the subsidy than with the cash transfer, however, as expected, he is better off with the cash. According to the notion of revealed preferences, when he bought (F_C, Z_C) with the monetary transfer, he could have bought (F_S, Z_S), but when he bought (F_S, Z_S) with the food subsidy, he could not have bought (F_C, Z_C). Consequently, (F_C, Z_C) is revealed as preferred to (F_S, Z_S).

The reason why this happens is that any kind of subsidy (or condition) constrains the poor, while the opposite occurs with the unconditional monetary transfers. Notice that in the explained model, P had to bias his purchases towards food when he received the subsidy, whereas with cash grants (without strings attached) he could buy whatever he prefers. It is thus this *freedom of choice* that allows beneficiaries to maximize their utility functions. The overall lesson learned from the revealed preference model is consistent with the argument that if individuals make rational decisions in a well-functioning competitive market, and the government really cares about the overall well-being of those in poverty, cash transfers should be unconditional.

Therefore, it is in the rational choice theory –and in the theory of well-functioning markets– that a large part of the arguments against the presence of conditions are supported. Samson, Van Niekerk and Macquene (2006) argue that the conditioning of cash transfers (presuming that the poor will not make rational choices) generates unnecessary distortions in the market (undermining household autonomy) and limits the capacity of the beneficiaries to maximize their utility. In addition, according to Barrientos (2011) and Hanlon et *al.* (2010), conditions may not be necessary since, in their absence, those in poverty would spend the money on food or investing in the human capital of their children and, if some of the money remains, they would invest it in productive activities that improve their income-generating capacity. They believe that poverty is fundamentally based on the lack of money, but it is not a matter of stupidity. If these arguments are true, conditionalities may not always have the positive results expected by their advocates and could even have *counterproductive* effects. This reasoning, which calls into question the true effectiveness of conditions in anti-poverty programmes, constitutes precisely the main theoretical argument for *not* conditioning a cash transfer.

There are other compelling theoretical arguments against conditionality, which are related to the actual cost-effectiveness of this policy mechanism. First, it could be argued that the financial and opportunity costs of complying with human development conditions are typically high for people living in poverty (Barrientos, 2011). Thus, even if the presence of conditionality were effective in some degree or had some positive effects, the costs of compliance for the beneficiary households should also be considered to determine if conditions are worth being implemented. It could be the case that poor households find conditions too difficult/expensive to comply with (for example, the schools might be too far away from home or the transaction/transportation costs might be too high), and therefore they may choose to give up the benefits that they need. In other words, if beneficiaries behave rationally, conditions can exclude some poor people from participating in social assistance initiatives.

Moreover, conditions might not work when public services are scarce or inefficiently provided. For instance, it may be that schools are low-quality (or hospitals are overcrowded), in which case conditionality represents a waste of resources for both the government and the beneficiaries. The government would be spending money from the anti-poverty budget in implementing and monitoring conditions that cannot be met, while beneficiaries would be wasting their time instead of healing, working or studying at home (Myamba & Ulriksen, 2016). Therefore, poor people may be making bad decisions *due to* conditionality because, as the rationalist perspective suggests, only 'freedom of choice' allows households to maximize their utility.

1.6.2 Conditioning Cash Transfers: Why Is It Necessary?

In spite of the arguments against conditionality, CCTs have been widely adopted as innovative forms of social assistance in developing countries (as discussed in page 26). However, there is no *conclusive* empirical evidence supporting the idea that the addition of conditions results in better programme performance. So, what justifications can be

given for conditioning a cash transfer? In the literature, there are at least three compelling theoretical arguments to believe that the inclusion of conditions is necessary in antipoverty programmes.

The first and perhaps the most important argument is that, as explained by the paternalistic approach (see page 30), poor households (and human beings in general) do *not* always behave rationally and they lack full information on the long-term benefits of education and health (Fiszbein et *al*., 2009). Research in behavioural economics suggests, for example, that people's everyday behaviour is not consistent with their own thoughts about the future (O'Donoghue & Rabin, 1999). This supposedly irrational behaviour and the presence of imperfect information may cause poor households' investment in human capital to be too low, compared with the level of investment considered "optimal" for them (Maluccio & Flores, 2004; Fiszbein et *al*., 2009).

If private decisions made by individuals in poverty situation are not adequate due to lack of capacity and information, then this supports the notion that policymakers and authorities must make decisions for them – because the government "knows better" what is good for the poor than those in poverty themselves. Therefore, it is argued that the presence of conditions is required to achieve the programme objectives and maximize the human development impact of cash transfers (Barrientos, 2011). In other words, the alleged inability of the poor justifies the presence of conditions as a policy mechanism to promote their "correct" decision-making and, at the same time, to prevent dependence on cash transfers (addressing the main concern of the welfare trap theory) through the introduction of a sense of 'co-responsibility' among those in poverty. Supposedly, conditioning the delivery of transfers prevents people from getting used to the state providing them money without asking anything in return, which –according to the ideas of welfare dependency– distorts the values and attitudes of people with respect to work or education.

The second theoretical argument for making cash transfers conditional is related with the almost unavoidable presence of market failures, and particularly, human capital

externalities. Some make the point that certain types of investments in human capital (e.g. education and health) generate positive externalities (also known as spill-overs) to societies (Fiszbein et *al*., 2009). These positive spill-overs arise when the activity of one agent impacts the performance of the others, leading to higher levels of human capital and well-being (or production) at the aggregate level. According to Adato and Hoddinott (2007), poor households do not take into account these externalities when making private decisions about human capital investment, and thus the aggregate level of human capital in the society might not be the socially optimal. Put another way, even if poor households' decisions about human capital investment are privately optimal, these might not be socially optimal because people do not consider possible spill-over effects.

The possible presence of positive externalities could justify governments' intervention conditioning cash transfers as an effective means of increasing human capital investment. There is some empirical evidence that supports the idea of spill-over effects in education and health. For instance, Moretti (2004) evaluates the magnitude of human capital investment externalities among industries in the same city. Using different measures of economic distance, he finds significant spill-over effects of education that are much larger when industries are more economically closer. However, it is necessary to mention that there is no conclusive evidence on the size of human capital externalities, as well as whether CCTs are the best way to correct for them and promote socially optimal investment decisions (Fiszbein et *al*., 2009).

Finally, the third theoretical argument for conditioning cash transfers is related to the *political economy* decisions required to fund redistributive policies and increase their public support (Myamba & Ulriksen, 2016). Since social assistance programmes need to be approved and financed by tax-payers (or international institutions), governments must make well-thought-out policy decisions to make them viable and well-endowed. These decisions are not taken by generous policy-makers; rather they are the result of usually difficult political economy processes. Besides, it is conceivable that income transfers targeted to the poor tend to receive limited political and public support, since only a small

share of the population enjoys the benefits, while the costs are borne by all taxpayers (Fiszbein et *al*., 2009).

In this context, it is likely that politicians and voters are more willing to accept transfers to those in poverty who are seen to be assisting themselves (through investments in their children's health or education) than to other poor people considered lazy or thoughtless. Thus, conditionality has the potential to overcome the possible stigma associated with social assistance payments since human development conditions are considered part of a social contract between beneficiaries and the government (Adato & Hoddinott, 2007). More specifically, conditioning cash transfers on certain behaviours considered socially desirable among the poor (such as investment in human capital) may increase public support for such programmes, which in turn increases the budget for income redistribution and makes possible the implementation of better social assistance practices. In fact, there is some evidence in the behavioural economics literature on fairness judgments suggesting that people are prepared to experience financial losses to reward behaviour perceived as socially fair or to punish behaviour perceived as unfair (Fehr & Gächter, 2000).

This political economy argument receives support from the empirical evidence on the politics of conditions in Brazil. Lindert and Vincensini (2008) find that most media criticism of the Bolsa Familia programme in Brazil is focused on the potential welfare dependency effect among those in poverty (i.e. welfare trap theory), and these critics are usually accompanied with reports that conditions are not being monitored properly by the government. However, they point out that most people who consider that the programme is *not* creating dependency; also think that the conditions attached to the programme are the main reason for this. The authors conclude that conditions played a major role in Brazil, acting as a legitimate response to the usual political constraints faced by policymakers and increasing public support among taxpayers.

1.6.3 The Conditionality Debate

Admittedly, there are quite convincing theoretical arguments both for and against the inclusion of conditions in human development income transfer programmes. As a result, the discussion on this topic in the field of social protection is far from over and, on the contrary, it has intensified with the passage of time. The debate on 'global CCTs' is now a very large and important strand of the social policy in developing countries scholarship (Adato & Hoddinott, 2007; Arnold et al., 2011). This is the case because, although there is a powerful tendency towards paternalism, the idea of conditionality remains highly controversial in principle and its actual effects in practice are still questionable. In fact, conditionalities are often seen as examples of policy mechanisms where moral ideas or principles (e.g. freedom and human dignity) may conflict with practical concerns (e.g. improve efficiency, prevent welfare dependency and safeguard political support).

A remarkable resource on the current 'conditionality debate' is that of Samson et *al*. (2006), *Designing and Implementing Social Transfer Programmes*. Based on a South African perspective, the authors recognize that designing effective conditionalities could be a very difficult task and for this purpose they provide a comprehensive guide. Although Samson et *al*. (2006) emphasise the value of conditions from a political and social perspective, they also acknowledge that it is critical to carefully evaluate all the possible consequences of conditionality in order to provide social benefits that exceed social costs. Another more recent contribution that squarely fits on these issues about conditionality is Myamba and Ulriksen (2016), who critically assess the presence of conditions in anti-poverty programmes using examples of social protection development and the current policy debate in Tanzania. According to the authors, conditionality has been firmly entrenched for a long time as a necessary policy mechanism in cash transfer programmes, but *only* to ensure the required political support while deliberately ignoring important *principled* and *practical* concerns, around which the debate on this topic takes place.

In the first place, the debate on conditionality has raised a number of ethical and moral concerns. Some make the point that the idea of conditioning a cash transfer is in

clear opposition to the fundamental principles of human freedom and dignity. This ethic-based perspective, commonly known as the *principled opposition* to conditionality, has been partly analysed in the emerging literature about behavioural interventions (see page 32). Seen from a basic perspective of human rights, given that all people are born equal in dignity and rights, we should have all the same freedoms and the same right to access welfare benefits whenever we need it. In fact, the Universal Declaration of Human Rights states that everyone has the right to social security and social protection (UN General Assembly, 1948). If the delivery of a social benefit is conditioned, it would be threatening to deprive the neediest of that 'universal right' if they fail to comply with certain conditions, and it would be acting directly against the individual liberties of the people to do what they see fit and spend their money as they wish.

In addition, the simple act of trying to influence the behaviour of social assistance beneficiaries implies that they are considered incapable or unwilling to make "good" decisions and spend the money "well", something that apparently can be perfectly done by other people (i.e. those who are not poor). In other words, there is an implicit understanding that the poor are not equal to the non-poor and, therefore, they should be treated differently by subjecting them to certain rules or restrictions in case they wish to access social protection (Myamba & Ulriksen, 2016). Supporters of the 'principled opposition' to conditionalities consider that, even if the implementation of conditions had positive outcomes for those in poverty (which is quite questionable), this widespread policy mechanism should not continue to be placed above the fundamental rights of the human being.

Apart from the principled concerns mentioned above, the conditionality debate has largely focused on important practical concerns about the *effectiveness* and *convenience* of attaching conditions to cash transfers. The basic idea of conditionality that the state knows better than its citizens what is the best way to use its scarce resources is indeed a very dubious assumption (Freeland, 2007). Until now, the true effects of linking conditions to anti-poverty programmes remain uncertain. Although there is a large amount of evaluative literature on CCT programmes, which mostly shows that the outcomes have

been positive for the beneficiaries (Rawlings & Rubio, 2005; Behrman et *al*., 2010), it is difficult to separate the effect of conditions from the effect of cash transfers (that is, the income effect versus the conditionality effect). The lack of sufficient empirical evidence in this regard makes it challenging to determine, for example, whether the same, better, or worst results could have been achieved without the presence of conditions (Freeland, 2007; Barrientos, 2013). Besides, this uncertainty over the effectiveness of conditions is reinforced by the fact that well-being improvements are also observed in places where cash transfers are made unconditional (Devereux, 2009; Ozler et *al*., 2010).

Despite all the academic interest in the relative effectiveness of conditionality, the empirical literature directly comparing CCTs and UCTs is surprisingly very limited. The first studies of this type were based on natural experiments (originated by failures in the implementation of conditioned programmes) or on structural models of household behaviour (Bourguignon et *al.*, 2003; Schady & Araujo, 2008; Paxson & Schady, 2010). The great majority of these non-experimental evidence coincided in concluding that behavioural conditions play an important role when it comes to obtaining better human capital outcomes, specifically in terms of schooling. It was only a few years ago that a series of controlled experiments, designed to identify the specific marginal contribution of conditionality, were performed with relative success. By assigning households to two different treatment groups (one that receives conditional cash transfers and another that receives unconditional transfers) and a control group (which does not receive transfers), different randomization-based studies found heterogenous results on the actual effects of conditions associated with the programmes implemented in some less-developed African countries (Devereux, 2009, Ozler et *al.*, 2010, Haushofer & Shapiro, 2013).

Although separately there are some quite extensive and detailed reviews of the existing evidence in the literature, both on conditional cash transfers (Parker et *al.*, 2008, Fiszbein & Schady, 2009, Adato & Bassett, 2012) and on unconditional transfers (Hanlon et *al.*, 2010), there are very few revisions that attempt to systematically and jointly compare the two types of programmes. Given how important and extremely useful it would be for policymakers to have such systematic reviews of the different CCT and UCT

interventions, it is rather surprising that there is currently a shortage of (direct or indirect) comparative studies. There is, however, one fairly comprehensive review of both types of cash transfers that is worth mentioning and discussing in depth below.

Baird, Ferreira, Ozler and Woolcock (2013) aim to synthesize and complement the existing studies –both experimental and quasi-experimental evaluations– on the relative effectiveness of conditional and unconditional cash transfer programmes in improving schooling outcomes in developing countries. More specifically, they conduct a systematic review of the evidence in order to assess the overall impact of each type of intervention on school-enrolment, -attendance and performance (i.e. test scores). Along with this main objective, the authors also intend to contribute to a better understanding of the role played by the different dimensions of cash transfer programmes, such as the intensity of conditionalities, the size of the transfer, the enrolment rate before the intervention and the level of coverage of the programme.

In principle, it seems quite likely that beneficiaries of CCT and UCT programmes spend their cash transfers differently and, therefore, obtain different schooling outcomes as well. One could expect, for example, that the beneficiaries of conditional transfers spend a greater percentage of the transfer money investing in the human capital (i.e. health and education) of their children, since they are forced to do so due to the presence of conditionalities. Obviously, this would imply the existence of an important market failure, by which households are not sufficiently capable of giving education the value it deserves. This failure would be corrected to some extent by transfers attached to behavioural conditions, which would encourage higher spending on education than unconditional transfers. On the contrary, it is also possible that the beneficiaries of both types of programmes have previously spent little on the education of their children simply because they did not have the necessary economic resources, in which case one would expect quite similar spending patterns and schooling outcomes in the households that received conditional transfers and those that received unconditional transfers.

In order to test which of these two hypotheses best fits the existing evidence, the comprehensive review by Baird et *al.* (2013) included data from 35 studies in developing countries (26 on CCTs, five on UCTs and four studies that directly compare conditional with unconditional transfers). Eligible studies included both experimental and quasi-experimental designs in different languages (i.e. English, Portuguese and Spanish).[9] Their main finding is that both types of programmes (i.e. conditional and unconditional) have a significant effect on school enrolment and attendance. For example, CCTs improve the likelihood of a child being enrolled in school by 41 percent (confidence interval: 27 to 56 percent) and UCTs increase the probability by 23 percent (confidence interval: 8 to 41 percent). This is, of course, when compared to children who do not receive transfers (i.e. control group). Although the effect sizes on the schooling outcomes seem to be always larger in average for CCTs compared to UCTs, the difference is *not* statistically significant (i.e. overlapping confidence intervals).[10] These results suggest that, irrespective of the type of programme, beneficiary households are not only using the transfers for smoothing consumption and satisfying basic subsistence needs, but also for investment in the human capital of their children. Moreover, the evidence shows that cash transfers could also modify households' behaviours without the need of conditions.

However, the authors realize that categorizing the programmes just into CCTs or UCTs overlooks the fact that "there is a great deal of variation in the intensity of the conditionality" (Baird et *al.*, 2013, p. 7). Therefore, in order to effectively capture the diversity of programme designs, they grouped the conditionality variable into three categories: (i) no conditionalities, (ii) some conditions, but no enforcement or monitoring, (iii) explicit, monitored and enforced conditions. Their findings indicate that type (i) transfers positively affect the likelihood of a child being enrolled in school by 18 percent (confidence interval: 5 to 33 percent), type (ii) transfers increase the probability by 25

[9] The sample was limited to publications after 1997 (i.e. year in which the Progresa/Oportunidades programme was implemented in Mexico).

[10] A meta-regression was carried out to assess differences between the effects of conditional and unconditional cash transfers. Although a small difference was estimated, it turned out to be statistically non-significant (p-value=0.18).

percent (confidence interval: 10 to 42 percent) and type (iii) transfers by 60 percent (confidence interval: 37 to 88 percent). The 95 percent confidence intervals (CI) for type (i) and (ii) transfers overlap, showing that UCTs and programmes with conditions that are not enforced or monitored have statistically similar effects (i.e. the difference is not statistically significant). On the other hand, the 95 percent CIs for type (i) and (iii) transfers are the only ones that do not overlap, showing that the impact on schooling outcomes of programmes with monitored and enforced conditions is higher than that of UCTs.

In the light of the evidence, Baird et *al.* (2013) conclude that the intensity of the conditions may play an important role with respect to the effect size on schooling outcomes. More specifically, when conditionalities are properly monitored and applied, the positive effects of CCTs on school enrolment and attendance would be slightly higher than those of the UCTs. Thus, they strongly suggest that the force with which conditionality is imposed determines in turn its measured level of effectiveness. Interestingly, apart from the intensity of the conditions, none of the other dimensions of cash transfer programmes (e.g. the size of the transfers) has a significant impact on the magnitude of the effect on educational outcomes. In addition, unlike what happens with respect to school enrolment and attendance, the effectiveness of both types of programmes to improve academic performance is apparently null. However, according to the authors, new and better research is needed that also takes into account medium and long-term educational results, such as test scores or the approval of the school year. Similarly, they consider that more research is necessary on UCTs since a clear limitation of their study is the few existing rigorous evaluations of this type of programmes.

As a result, there are still very different points of view about the effectiveness of attaching conditions to cash transfers. According to the rationalist perspective, people would achieve even better results if they could act freely (i.e. without conditions or constraints), since individuals (more than governments) know best how to use their own money and resources (Sen, 2001; Hanlon et *al*., 2010). On the other hand, the paternalistic perspective maintains that poor people make bad decisions and that they cannot take care of themselves, so the best way to achieve the desired results is by having the authorities

make decisions for them – that is obliging them to comply with conditions (Bastagli, 2008; Fiszbein et *al*., 2009). Although the practical value in terms of effectiveness of conditionalities (i.e. the degree to which conditionality is successful in producing the desired results) is still unclear, this has been a topic of debate that policymakers commonly overlook when designing policies and implementing social protection programmes.

Finally, the debate has revolved around practical concerns about the convenience (i.e. cost-effectiveness) of conditionality, considering not only the costs of compliance for beneficiaries, but also the high administrative/opportunity costs that the enforcement and control of conditions represent for the government (along with the poor quality of social services in developing countries). One might think that conditional programmes are much more difficult to administer than unconditional ones, and that the establishment of control mechanisms can be very expensive (Samson et *al*., 2006). There is also the common consideration that it is the most vulnerable who will find it more expensive to comply with the conditions; and that the areas typically inhabited by the poorest are often those where public services are most precarious (Freeland, 2007).

Note that if the financial and opportunity costs for people living in poverty are too high, human development conditions may not even be effective in modifying people's behaviour and they could deprive of benefits to those most in need (Myamba & Ulriksen, 2016). According to the rationalist perspective, people may prefer to forego transfers if the cost of sending their children to school (taking into account transportation and the loss of a key source of income) exceeds the value of the promised monetary benefits. On the other hand, if (due to ignorance or irrationality)[11] those in poverty decide to modify their behaviour so as not to renounce the transfers, anyway the short-term expected outcomes (i.e. increase and smooth households' income level) would not be achieved and the conditions would end up worsening their current income situation.

Even assuming that initially the costs perceived by the poor are low and that conditions are effective in modifying beneficiaries' behaviour (be they rational or

[11] As the paternalistic perspective suggests.

irrational), this does not guarantee at all that *conditioned* decision-making will always be privately or socially optimal, given the frequent presence of important negative externalities (e.g. scarce supply of public services). If poor households have problems accessing public services or if these services are of low quality, additional financial and opportunity costs are generated that parents would not be considering when making human capital investment decisions. In this sense, it is important to consider that the desired result of conditionalities is not (or at least should not be) simply to modify the behaviour of people according to an obviously subjective criterion. It is assumed that changes in behaviour are only a means for people to make "good" decisions, which could not be made if they are induced to perform certain actions (e.g. send their children to school) when the provision of public services is inadequate (e.g. classrooms are overcrowded because there are not enough teachers). Thus, conditionality not only does not guarantee good decision-making by those in poverty, but, in some cases, it could be the reason why they make incorrect investment decisions (in education or health) or choices that are not in their best interest.

In the case of many developing countries, it is almost a fact that social assistance beneficiaries' access to public services is seriously affected by their scarce or inadequate supply. In fact, there are some qualitative studies suggesting that beneficiaries of social programmes often try to comply with the conditions, but that they cannot access education or health-care services due to their inadequate provision (Myamba & Ulriksen, 2016). Therefore, it is very likely that –at least in low-income countries– it is preferable to invest more money in improving the provision of social services, instead of spending it on controlling compliance with conditions. In other words, spending money from the tight anti-poverty budget in enforcing conditions may not be justified when social services offered by the government are insufficient. As Davis, Gaarder, Handa and Yablonski (2012) argue, conditionality requires a significant administrative investment that could be better capitalised in building up educational and health-care resources. The low quality of public services could be a more serious problem than the fact that the beneficiaries of social assistance do not use them as they apparently should.

The extent and intensity of the debate on conditionality contrasts deeply with the enormous popularity of CCT programmes in developing countries. In fact, over the last twenty years, conditions have been firmly rooted in the social protection policies of most Latin American countries. A possible explanation may be that policymakers often prefer to focus on the supposed and promising practical benefits of CCT programmes (i.e. generate human capital externalities, obtain political support and prevent welfare dependency), rather than worrying about moral ideas, ethical principles and unresolved effectiveness concerns. Myamba & Ulriksen, (2016) argue that in developing countries' policy debates it is uncommon to address the issue of conditionality from the more intricated perspectives of cost-effectiveness, freedom of choice and human dignity. On the other hand, concerns related to the political economy of financing these redistributive policies are always present.

For this reason, it is not surprising that –at the time of making welfare policy decisions– the inclusion of conditionalities is not only little questioned but is often considered necessary to obtain funding resources and political commitment. In this sense, policy recommendations and financing offers formulated by multilateral organizations, which have traditionally favoured the implementation of CCT programmes, have a great deal of influence. In fact, international financial institutions, such as the World Bank, are considered the main defenders of the ideas in favour of conditionalities (Freeland, 2007). Tina Rosenberg, the Pulitzer Prize-winning writer for the New York Times, said during a debate at the World Bank that: "part of the reason why these (conditional) programmes are becoming popular is the support of institutions like the World Bank and the Inter-American Development Bank" (World Bank, 2009)

Through comprehensive institutional reports and carefully designed impact evaluations, international development organizations argue that those in poverty maintain incorrect ideas about the profitability of investing in the human capital of their children; and also, they emphasize the positive outcomes of CCT programmes around the world (Fiszbein et *al*., 2009). In consequence, they consider beneficial and strongly recommend conditioning cash transfers to the use of specific health and education services by the

beneficiaries. This is in addition to offering technical assistance and financial support to the developing countries for the implementation of this type of programmes exclusively. For example, Axel van Trotsenburg, Vice President of Financing for Development at the World Bank, said in 2008 that they would financially support the Colombian CCT programme with a large loan, but, above all, they will be actively involved in the design of the programme, in its control and even in its evaluation (World Bank, 2009). In the words of Freeland (2007), "the term conditional (in CCTs) is redolent of the conditionalities imposed by IMF, World Bank and other donors when making loans or implementing budget support programmes".

For many years, the dominant discourse in policy debates has been that it is necessary to "help" those in poverty in their decision-making, break the inter-generational transmission of poverty and reduce the possibility that transfers are generating dependency through the permanent implementation and control of conditions. However, as discussed in this chapter, there are still serious doubts regarding the veracity of these theoretical benefits and the supposed effectiveness of conditionality. Therefore, the only certainty about this issue is that, in practice, concerns related to the political economy of funding cash transfer initiatives –and political calculations seeking taxpayers' support– tend to be placed above concerns about fundamental human principles, programmes' efficiency, and even the most stylized economic theories (Arnold et al., 2011; Myamba & Ulriksen, 2016). At least in the design of social protection policies in Latin America, the presence of conditionalities as a "necessary" policy mechanism has been taken for granted for too long.

Meanwhile, the social policy debate on conditionality has intensified worldwide in recent years, especially in the academic spheres, where the idea of conditioning cash transfers continues to be highly controversial. There is no absolute truth or an indisputable argument on this topic since the design of social programmes responds to the characteristics and scope of poverty in each particular context. Decisions on conditions should be considered carefully and, even if conditionality were shown to work in some environments, it is very unlikely that it will do so in different places or at other times

(Freeland, 2007). An interesting case to exemplify this long-standing policy discussion is that of Ecuador, where conditional and unconditional cash transfer programmes have coexisted for about a decade, demonstrating that even the policymakers of the same government have not yet been able to reach an agreement on the structure and characteristics that social assistance practices must have to be more effective and, in this way, be able to lift more people out of poverty.

1.6.4 Work-related Conditionalities

As discussed throughout the chapter, there has been a broad debate in academic and policy circles about the appropriateness, nature (i.e. conditional or unconditional) and generosity (i.e. amount of the transfers) of social transfers for the poor and vulnerable that should be put in place in developing countries, but also indeed, across the world. For now, CCTs have taken the lead as the most popular anti-poverty programmes globally. Apart from the *traditional* CCT programmes, whose conditions are related to health and education, there are also cash transfer programmes that impose conditions related to work – mainly due to the strong concerns of policy-makers and academics about possible reductions in adult labour supply that can result from social assistance practices.

Although economic theory predicts that income transfers will have an impact on the labour supply of beneficiaries, the possible effects are difficult to predict, especially in the context of anti-poverty cash transfers (Barrientos & Villa, 2013). In fact, there is still much controversy about the scope of the work disincentive effects and whether work-related conditions are necessary or not (Jung & Smith, 2007). As a result, there is a wide range of policy logics and policy instruments to address these potential issues of social assistance. The selected approach can change very quickly even within a given country (sometimes it just takes a change of government). Broadly speaking (and with the sole intention of being illustrative), we can note that developed and less-developed countries

have adopted two opposing choices/approaches with respect to work-related conditions in the last few decades.

In the United States, for example, the perception that traditional CCT programmes encouraged inactivity among those in poverty and promoted dependency on public assistance inspired the 1996 Personal Responsibility and Work Opportunity Act (PRWORA) and the implementation of transfers that are exclusive to the working poor (Fiszbein et *al.*, 2009). These transfers impose work-related requirements, strengthened eligibility conditions and time limits (Scholz and Levine, 2001). In contrast, most Latin American and some continental European countries continue to rely heavily on traditional programmes, which are almost always unconditional to work, time-unlimited, and not specifically targeted at households with children (Bargain & Doorley, 2011).

Like in the United Stated before 1996, traditional CCT programmes in developing countries have been accused –especially by those in favour of the paternalistic and welfare trap approaches– of creating strong work disincentives (World Bank Group, 2015; Barrientos & Villa, 2013). This may occur as consequence of reduced job search intensity of transfer recipients or less willingness to accept a job if the expected wage is only marginally higher than the social transfers (Blank, 2003; Guzi, 2013). According to the welfare dependency perspective, poor people can become completely dependent on benefits, with little or no hope of self-sufficiency (see page 28). On the other hand, the paternalistic approach would point to work-related conditionalities as a necessary element to ensure that welfare programmes have desirable effects on labour supply. Considering these ideas, it is not surprising that the impact of CCTs on labour market participation has become a key research topic and, therefore, has received enormous attention in recent years (World Bank Group, 2015).

The available evaluative literature of the programmes implemented both in developing and developed economies emphasizes the *various* potential impacts on the labour market engagement of the beneficiary households. Therefore, it is important to keep a close eye on the specific 'labour supply' effects because their accurate estimation

is key to the correct evaluation of any income transfer programme. According to Barrientos & Villa (2013, p.3), "antipoverty programmes are not welfarist. […] An antipoverty income transfer leading to a proportional reduction in labour supply, and therefore income, could well be welfare enhancing, but would be considered a failure in its own, non-welfarist, terms".

Numerous methods have been used to estimate the disincentive effects of social assistance programmes (with and without work-related conditionalities) on labour market behaviour. The evaluative literature is vast for programmes implemented in the developed world and most of the research has focused on the cases of the United States and the United Kingdom (Moffitt, 1992; Krueger & Meyer, 2002). The methods used by academics include structural approaches (e.g., Meyer & Rosenbaum, 2001), controlled experiments (e.g., Bloom & Michalopoulos, 2001) and natural experiments that have exploited important tax-benefit reforms to identify behavioural parameters (e.g., Blundell et *al*., 1998). Overall, the size and significance of the estimated work disincentive effects have been quite heterogeneous in developed countries (Moffitt, 1992; Jung & Smith, 2007; Fiszbein et *al*., 2009).

There is much less evidence available for traditional CCT programmes (which are unconditional to work) in developing countries and even continental Europe. Besides, due to the lack of major reforms in this matter, most of the evidence in these countries comes from estimates of controlled experiments and structural models –which are necessarily based on strong distributional assumptions and have not often been validated against natural experiments (Bargain & Doorley, 2011). Skoufias and Di Maro (2006) study the effects of Mexican Oportunidades on adult work and they do not find evidence of a negative impact. Similarly, Edmonds and Schady (2012) suggest that the Ecuadorian BDH had no disincentive effects on labour force participation. In general, the estimated effects of CCTs on adult work in developing countries appear to be only marginal (both positive and negative) or inexistent (Fiszbein et *al*., 2009). One of the few exceptions is Maluccio and Flores (2004), who report that male adult labour supply was significantly and negatively affected by the RPS programme in Nicaragua, although there was no effect on

women. Another important paper is that of Barrientos and Villa (2013), who relying on a regression discontinuity design, find that a CCT programme in Colombia had considerable positive effects on labour supply outcomes.

1.7 A Guide for Enquiry: Research Questions

A series of key concepts from social protection theory and different theoretical perspectives about the issue of conditionality have been presented. These will be used to frame the effectiveness comparison of the two most important cash transfer programmes –in terms of coverage and budget– offered in Ecuador: the one with a traditional standard design and the recently implemented programme in which families receive a lump sum payment without attached conditionalities. Building on the above theoretical pillars, as well as the empirical research literature and social assistance policy developments reviewed throughout this chapter, this final section outlines a linked sequence of three research questions which guide the enquiry of this research:

> Q.1 Do the BDH and CDH cash transfer programmes affect the levels of well-being, schooling and unemployment of those in poverty?
>
> Q.2 Does conditionality make a difference in terms of the efficiency of the programmes?
>
> Q.3 Is it effective to set aside conditionality and trust the poor with lump-sum cash transfers?

CHAPTER 2
Cash Transfer Programmes in Ecuador

2.1 Introduction

In spite of the large number of cash transfer programmes available in many developing countries, surprisingly, there is very little variation in their structure (J-PAL, 2012; Arnold et *al*., 2011). Most programmes are inspired by the Mexican model -first known as Progresa and now as Oportunidades- and have maintained the same basic configuration: families in poverty or extreme poverty, which have one or more minor children, receive monthly monetary or in-kind benefits *if and only if* their children receive regular medical check-ups and meet specific attendance targets at school (Levy & Rodríguez, 2005).

Certainly, while conditional cash transfer (CCT) programmes have become an important tool for poverty alleviation in less developed nations, the challenge today is to make these programmes evolve into more efficient mechanisms that take more poor households out of poverty and provide them with social and economic independence, making more people self-sufficient (Sadoulet & de Janvry, 2004). As an attempt to achieve this goal, some important variations to the typical structure of cash transfer

programmes have been implemented in the South American country of Ecuador over the last decade.

Ecuador is a lower-middle income country, characterized by high levels of poverty and inequality. In 2011, its per capita GDP was 1,870 US dollars (constant 2000 US dollars). According to data from the Ecuadorian Central Bank, around 16 percent of the population lives under the extreme poverty line, and more than 35 percent lives under the poverty line (BCE, 2012).[12] Even though, poverty in Ecuador has actually decreased over the last decade, inequality has not presented significant changes. The Gini coefficient registered in 2008 was 0.49, while in March 2012 it was around 0.45. From 2000 to 2011, the richest 20 percent of the population concentrated more than 50 percent of the national GDP, whereas the poorest 20 percent not more than 5 percent of it (BCE, 2012; INEC, 2012).

According to the available data from the World Bank's Atlas of Social Protection - Indicators of Resilience and Equity (ASPIRE) database, Ecuador's total social public expenditure maintained a growing trend in the first decade of the 21st century, reaching about 9.6 percent of the country's GDP in 2010.[13] Amongst this public spending, one of the components that grew the most was the spending on social assistance programmes, going from 0.6 percent of GDP in 2000 to 1.8 percent in 2010. Between 2005 and 2010, Ecuador incurred an (average) expenditure on social assistance programmes of around 1.45 percent of GDP, which at that time placed this developing Latin American country above the average expenditure among the countries in its region (i.e. 1.3 percent of regional GDP).[14] Appendix A examines the evolution of Ecuador's public spending on social assistance programmes and its performance in terms of poverty and inequality reduction (see page 399).

[12] The poverty threshold, or poverty line, is the minimum level of income deemed adequate in a given country. In Ecuador, the poverty line and the extreme poverty line have been around 2 and 1 US dollars a day respectively.
[13] This percentage includes public spending on education, health and social assistance.
[14] More information about ASPIRE database available at http://datatopics.worldbank.org/aspire/home.

The institutional structure of the Ecuadorian welfare state has recently shifted from a fragmented system of social protection to a more integrated network within the Ministerio de Inclusión Económica y Social (MIES) (Gutierrez et. al., 2013). Over the past 15 years, the Ecuadorian Government has been trying to help the most vulnerable people in the country by developing a comprehensive strategy called Programa Red de Protección Social (PRPS) (Social Protection Network Programme), which has a traditional CCT programme –known as the Bono de Desarrollo Humano– as its main component.

The Ecuadorian social protection network accompanies and assists all groups in situation of vulnerability or exclusion by delivering different types of monetary transfers. These transfers have been consolidated under three kinds of interventions: (i) unconditional cash transfers (e.g. Crédito de Desarrollo Humano programme); (ii) conditional cash transfers (e.g. Bono de Desarrollo Humano programme); and (iii) interventions of economic inclusion and contingency to different risks (e.g. Retirement Bonus programme). The main goal of the Ecuadorian PRPS is to ensure the rights of vulnerable social groups (such as access to basic services, education and health) so that the families that receive this assistance can improve their quality of life and can get out of poverty (PRPS, 2013).

In order to achieve these objectives, the MIES –through its social protection agenda– offers cash transfers through five specific programmes: Bono de Desarrollo Humano (BDH), Crédito de Desarrollo Humano (CDH), Cobertura de Protección Familiar (Family Protection Coverage), Pensión para Adultos Mayores (Pension for Older Adults) and Pensión por Discapacidad (Disability Pension). The last two programmes were implemented only a few years ago and are the ones with the lowest coverage levels. The purpose of the Pensión para Adultos Mayores programme is to protect the elderly population without access to social security or any other kind of insurance, while the Pensión por Discapacidad programme is aimed at people with disabilities and does not impose an age limit for beneficiaries. According to data handled by the Ecuadorian government, these programmes covered 600,000 older adults and 115,000 disabled people

respectively in 2013.[15] Moreover, the BDH and CDH are the most important cash transfer programmes in Ecuador; and are the focus of this thesis.

One of the key elements for the integration of these social assistance programmes into an effective social protection network has been the standardization of their databases, giving rise to a true complete and focused information system that provides more timely data (Gutierrez et *al.*, 2013). In this way, it has been possible to effectively monitor, both individually and at the household level, any potential beneficiary of the cash transfers. Additionally, this has allowed the same beneficiary family to access more than one programme at a time, thus protecting it from various social risks (including living in poverty, poor health, old age, disability and those associated with natural disasters).

Note in this regard that, speaking in general terms, social assistance programmes, service networks and social protection systems should always have good information, registration and monitoring systems that can be used to compare the initial and present situation of the (current and potential) beneficiaries, monitor progress and coordinate responses among policy providers (Jara et *al*., 2013). Therefore, the development of more integrated information systems, together with more efficient operational models, represent some of the basic conditions that Ecuador and other developing countries must meet before non-contributory social security systems can be implemented in a future.

However, perhaps the most important changes in Ecuador have to do with the structure and characteristics of the cash transfers interventions. More specifically, there have been significant variations in relation to the typical element of conditionality and the target populations of the different programmes offered in the country. Cash transfers in Ecuador have incorporated in recent years various types of beneficiaries normally not considered eligible for such programmes in other developing countries (Cecchini & Madariaga, 2011). Nowadays, access to benefits is possible for more categories of people in a situation of poverty and vulnerability, such as unemployed adults, people with

[15] See MIES (2013) for a more detailed explanation of the different social assistance programs offered in Ecuador.

disabilities, older adults, and poor families *without* minor children. In addition, as will be analysed in depth later, a social assistance programme has been implemented a few years ago that provides unconditional lump-sum (non-refundable) monetary transfers for productive investments, which represents one of the first interventions of this kind in the entire region and possibly in the world.

2.2 The Bono de Desarrollo Humano Programme

The Bono de Desarrollo Humano (Human Development Bonus), commonly known as BDH, was officially launched in 2003 and is the social assistance programme in the country with the highest level of coverage.[16] The BDH is mainly a direct subsidy that provides 50 US dollars cash transfers per month to families in extreme poverty. Unlike other programmes in the region, households without minor children or teenagers are also eligible to receive the benefits.

The targeting mechanism of the BDH is a *proxy means test* of welfare level (i.e. Welfare Index), an indirect method to measure the income of families, that is estimated based on the socioeconomic household information of the Sistema de Información del Registro Social (SiiRS), which is managed by the Ministerio Coordinador de Desarrollo Social (MCDS) (Ministry Coordinator of Social Development). This household information is collected through questionnaires, applied directly at home or at strategic points in some particular cases (due to geographical issues), as part of a national survey of potential programme recipients called Registro Social (Registro Oficial, 2009). The BDH target population at the moment are only families around the first quintile of the Welfare Index, known also as the SELBEN (Sistema de Selección de Beneficiarios de Programas Sociales) Index or poverty index.

[16] In 2013, this cash transfer programme reached a coverage level of about 31 percent of the total population, which meant that at that time the BDH programme was the social assistance initiative with the highest level of coverage in the region (Gutierrez et *al.*, 2013).

The BDH programme seeks, in the short term, to guarantee a minimum level of consumption in poor families, and in the medium and long term, to promote investment in human capital of those under 18 years of age to prevent the inter-temporal transmission of poverty (VAMS, 2014). In order to achieve these goals, the BDH provides cash transfers to the most vulnerable households in exchange for compliance with certain *conditionalities* in education and health. Therefore, in theory this programme maintains the same basic structure as traditional CCTs. It provides monthly payments on the condition that beneficiary families take children under 5 years old to at least one health check per year (to ensure their normal growth and development) and send children between 6 and 18 years old to school on a regular basis (to ensure continuous school attendance and reduce child labour) (Schady & Araujo, 2006; MIES, 2013).

Although the BDH programme clearly establishes the 'co-responsibilities' or conditionalities that families must meet and the penalties for non-compliance, according to different programme evaluations and institutional reports, during the early stages of the programme, the process of monitoring and controlling conditionalities was only partially carried out and the sanctions established by the rules of operation were rarely applied (Martinez & Rosero, 2007). According to the institution in charge of the BDH, the control of compliance with the conditions has hardened considerably since the beginning of this decade (MIES, 2013b).

During its first ten years of implementation, the BDH programme can be said to have continued to evolve positively: improving the targeting mechanisms, reducing focalization problems, increasing the level of coverage and expanding the target population (i.e. households qualified as eligible) (Gutierrez et. al., 2013). Since 2014, this important CCT programme –originally aimed at all poor households in the country (i.e. those in the first and second poverty quintiles)– adjusted its target population only to households living in extreme poverty (i.e. those in the first poverty quintile). The reasons for this significant change in the eligibility conditions are not so clear, but could be due to cuts in social assistance spending (see Appendix A) and to the fact that, according to the MIES, some leakage problems were detected (Martinez et *al*., 2017). In any case, a

significant amount of beneficiaries (about 60 percent) were withdrawn from the programme in late 2014.[17]

The BDH has been established as the cornerstone of a comprehensive social protection network that addresses several dimensions of social risk, and not just the issue of poverty. In addition to the delivery of a monthly cash transfer to each beneficiary household, the programme provides non-contributory pensions to those households with elderly and disabled persons. Between 2008 and 2014, the usual number of BDH beneficiary families had oscillated between 1 million and 1.2 million. In fact, the BDH Programme Report issued in 2013 indicates that the programme at that time covered about 1.1 million Ecuadorian households with an annual government investment of 484 million dollars (MCDS, 2013). Nowadays, the BDH still covers about 450,000 Ecuadorian households (2.7 million people) with an annual government investment of about 403 million US dollars, which represents about 0.4 percent of the GDP (Martinez et *al*., 2017).

Considering that the BDH was at some point one of the largest CCT programmes in Latin America in terms of percentage of beneficiary population (i.e. 31 percent of the total households in 2013) and the percentage of GDP that its budget represented (i.e. above the regional average until 2010), it is not surprising that it has attracted the attention of a number of researchers. In fact, due to its high incidence in the development of social policies in the country and the percentage of social assistance spending that it represents, the BDH has been one of the most studied social programmes in the region. The results of the main impact evaluations suggest that the programme has had some important positive outcomes such as the reduction of monetary poverty (León et *al*., 2001), the increase of school enrolment (Schady & Araujo, 2006; Oosterbeek et *al*., 2008), the reduction of child labour (Martinez & Rosero, 2007; Schady & Araujo, 2006; Dobronsky & Rosero, 2007) and the reduction of the cognitive development gap between

[17] Note that recent changes in the eligibility conditions do not affect in any way the results of this thesis, since the individual- and household-level data (with which the impact evaluations are carried out) correspond to the Registro Social surveys of 2008 and 2013-14.

disadvantaged children and higher-resource peers (Paxson & Schady, 2007; Ponce & Bedi, 2008).

Schady and Araujo (2006) employ an experimental method on a sample of 1,391 households to estimate the effects of the programme on school enrolment and child employment. The sample included children of school age (from 6 to 17 years of age), who were randomly assigned to treatment and control groups. The baseline survey was collected in 2003 and the follow-up survey in 2005. That is, the impact was evaluated after a period of around two years. The authors show that the BDH had a positive impact on school enrolment and a negative impact on child employment. Specifically, the findings suggest an 8.6 percent increase in school enrolment for children belonging to beneficiary families. Moreover, the beneficiary households experienced a reduction in child labour of about 17 percent. The authors emphasize that the estimated results are better than those of other conditional transfer programmes in the region.

Similarly, Oosterbeek, Ponce and Schady (2008) also examine the impact of the BDH on school enrolment, but using a regression discontinuity design. The data for the estimation were obtained from the same baseline and follow-up surveys collected by the Pontifical Catholic University of Ecuador between 2003 and 2005. Therefore, the sample consisted of a similar number of children than the previous study (i.e. 3,004 children belonging to 1,309 families). The authors show that for the first poorest quintile, the increase in school enrolment was of between 75 and 85 percent, while there is no clear effect for the second quintile.

On the other hand, the estimated impacts of the BDH programme on child malnutrition (León & Younger, 2007), food consumption (Ponce, 2008), attendance at health centres (Paxson & Schady, 2007) and children's general health conditions (Paxson & Schady, 2007) have been somewhat limited. Paxson and Schady (2007) evaluate the results of the BDH on infant health (haemoglobin levels, height and fine motor control). The authors used a randomized experiment on a sample composed of 1,479 children aged 3 to 7 years belonging to 1,124 families. The baseline survey was conducted in 2004 and

the follow-up survey in 2007. Their results suggest positive (but small) BDH effects on the health status of the children. For example, for the fine motor control variable, the results improved by 16 percent of a standard deviation for the treatment group versus the control group. In addition, it was found that the impact of the BDH was greater in the poorest families, and also that it increased the possibility that children had better nutrition and received medical treatment in case of stomach infections. However, no effect on attendance at health centres was demonstrated in the study. Moreover, Ponce (2008) finds that families around the eligibility cut (between the quintiles two and three) receiving BDH transfers spend on average 25 percent more on food than families who do not receive the transfers.

Remarkably, most of the existing impact evaluations have analysed the effects of the BDH programme using experimental methods or regression discontinuity designs with experimental data. In the evaluative literature, there are not many important studies that have used alternative quasi-experimental methods with secondary data, nor that evaluate the impact of the BDH on the overall welfare level or labour supply of the poor. In addition, almost all the evaluations were carried out with data from the first half of the previous decade, despite the fact that the most significant changes in the programme – with respect to the amount of the transfers and the number of beneficiaries– have occurred in the last decade. In fact, for the most recent periods there are no evaluations on the effectiveness of the programme. Appendix B presents a complete list of the main impact evaluations of the BDH (see page 411).

2.2.1 Evolution of the Bono de Desarrollo Humano

Until the late-1990s social protection practices in Ecuador were restricted to the services that the State provided on social security (namely, pensions and healthcare for formal workers), in addition to the traditional systems of education and health. At that time, these services were not constituted as a real network of social protection (or safety net) that: i)

would have been useful as a means to alleviate poverty; and, ii) would have served to mitigate the effects of the continuing economic crisis and natural disasters that affected a large part of the Ecuadorian population during those years (Martinez & Rosero, 2007).

It was only in 1998, within a context of pre-crisis, that the Ecuadorian Government incorporated a compensatory policy due to the elimination of important subsidies on cooking gas, gasoline, diesel fuel, and electricity. Specifically, a programme of direct cash transfers (called Bono Solidario) was implemented for the first time in the country. This programme was not initially conceived a mechanism for promoting human development and improving social protection, and, much less, as way to encourage investment in human capital (Schady & Araujo, 2006). Quite the opposite, the Bono Solidario programme was originally intended merely as a temporary compensatory measure for poor households due to the elimination of gas and electricity subsidies (Larrea, 2013).

However, given the deep economic crisis that Ecuador was going through in the period 1999-2000, which also constituted the main cause of the official dollarization of the economy, the Bono Solidario programme became the second most important component of social expenditure in the country (after expenditure in education), and the main tool to fight the effects of the crisis and protect the most vulnerable people -due to the loss of purchasing power of the wages and the high levels of unemployment during those years (Martinez & Rosero, 2007).[18]

The Bono Solidario functioned as a pure income transfer programme in which the State delivered monthly payments equivalent to 15 US dollars to three population groups: i) mothers with at least one child under 18, whose monthly family income did not exceed 1 million sucres (40 US dollars) and who were not affiliated to social security; ii) people over 65 years old, whose monthly family income did not exceed the equivalent to 40 US dollars and did not perceive a fixed monthly salary; and iii) people between 18 and 65

[18] The Bono Solidario represented up to 11 percent of the total social expenditure in those years.

years who had a degree of disability of at least 70 percent, according to the state's disability classification standards (Vos et *al*., 2003).[19]

The mechanism of beneficiary selection was based on *self-targeting,* but "surprisingly", there was no information about errors of focalization (namely, leakage and under-coverage) in the initial programme documentation (Martinez & Rosero, 2007). Actually, it was not until 2000 that the Bono Solidario was finally audited by governmental and academic institutions. The study of Vos, Ponce and León (2003) concluded that the self-targeting mechanism used, although progressive, suffered from under-coverage problems regarding the poor and leakage problems to the non-poor (about 42 percent of the transfers was channelled to middle income households). Moreover, an audit conducted between August 2000 and July 2001 revealed late payments to programme beneficiaries, a substantial surplus of cash intended to reach poor households still in the banks, and even the receipt of transfers by deceased people (De la O, 2015).

Most of these irregularities could be explained by the fact that there was no institution responsible for monitoring the quality of the information. As a result of this lack of control, the programme database was full of inconsistencies and some of the poorest families in the country were not even enrolled in the programme (De la O, 2015). Therefore, once the national economy somewhat improved in 2003, and since this programme assisted a large part of the Ecuadorian population (about 45 percent of the total families), the Bono Solidario was ultimately replaced by the current Bono de Desarrollo Humano. By combining the Bono Solidario and the Beca Escolar programme, an important shift occurred in the conceptual structure of social assistance practices in Ecuador.[20] According to Martinez and Rosero (2007), the axes on which this transformation was based were the following:

[19] Until the moment it was replaced by the BDH programme (that is, from 2003), the Bono Solidario programme covered more than 1 million beneficiary families (Vos et *al*., 2003).

[20] The Beca Escolar programme was the first attempt in Ecuador to offer monthly cash transfers to poor families with children –between 6-15 years– old in exchange for families enrolling their children in school and keeping them attending classes. (Martinez & Rosero, 2007).

a. Re-focalization of the programme through the beneficiary selection system known as SELBEN.
b. Application of formal conditions for the delivery of cash transfers, which are related to the behaviour of families in terms of education and health of their children.
c. Inclusion of an evaluation component that was designed by programme administrators in conjunction with the World Bank.[21]

This process of transformation and re-focalization of the CCT programme, which took place mostly in 2003, represented a key expansion of the social protection apparatus in Ecuador. It allowed the improvement of the targeting mechanisms and the continuous growth of the coverage level of the BDH programme (and other social assistance initiatives) for several years afterwards.

2.2.2 Target Population and Targeting Mechanism

In the context of public policy, *targeting* is the act of directing public resources, in the form of subsidies or transfers, to specific population groups in order to achieve certain policy objectives. Normally, subsidies and transfers provided by governments as part of different social programmes are targeted to those in poverty, but there are also programmes that select their beneficiaries based on other criteria, such as gender or ethnic origin. According to the literature on this topic, there are some different methods for targeting subsidies. For example, they can be targeted on the basis of specific individual (or group) characteristics, or by simple self-selection of beneficiaries. Therefore, in practice, the selection of the most appropriate targeting method will depend, among other

[21] The internal validity of this official impact evaluation was compromised by government's decisions and involuntary mistakes. Only 78 percent of the treatment group (and 42 percent of the control group) received the cash transfers because the information on beneficiaries' assignment was not received by the programme operational staff on time (Schady & Araujo, 2006).

things, on the costs of implementation, administrative viability and political feasibility (Bitrán & Muñoz, 2000).

Note that it is important to consider both the target population of the BDH programme and its targeting mechanism when constructing the sample used for the programme's impact evaluations (see page 189). Only in this way, one makes sure to take into account only the beneficiaries and the potential beneficiaries of the programme for their comparison (leaving aside people who are not entitled to receive the cash transfers). In addition, it is necessary to consider the changes in the beneficiaries selection criteria that occurred in 2003 and 2014, since these define the target population during the evaluation period (i.e. 2008 to 2014). As will be seen in Chapter 5, individuals in the sample are selected bearing in mind the modifications in the BDH beneficiary identification process that are explained below.

As previously mentioned, the instrument used for targeting and selecting beneficiaries of the programme is the Registro Social process, which collects individualized socio-economic information and also serves to identify potential beneficiaries of other social assistance programmes and projects offered by the Ecuadorian government. The targeting process is carried out by the MCDS in two main stages.[22] The first is a geographic identification of the poorest *census sectors* or districts in the country, while the second phase consists of conducting household-level surveys in the previously identified census sectors (Martinez et *al*., 2017).

The selection of the poorest census sectors is carried out using the cartographic division of the country designed by the Instituto Nacional de Estadísticas y Censos (INEC) (National Institute of Statistics and Census)[23] together with the Index of Unsatisfied Basic Needs (UBN, or NBI from the Spanish acronym) developed by the Andean Community

[22] The coordinating ministries in Ecuador (e.g. MCDS) have a higher rank than the other ministries and were established with the purpose of joining efforts in strategic areas of development, such as the fight against poverty.

[23] Most official and non-official surveys use this division to achieve national representativeness (INEC, 2013)

of Nations.[24] Specifically, the selected census sectors are those where the incidence of poverty (i.e. percentage of poor households) is greater than 50 percent. For example, in the last survey of socioeconomic information -made prior to the 2013 Registro Social-, 24,482 poor census sectors were identified from a total of 42,649 sectors into which Ecuador is currently divided (MCDS, 2016b).

Once the poorest census sectors have been selected, the second stage consists in conducting household- and individual-level surveys to build a complete registry for each family that resides in the chosen areas. This is done with the final purpose of objectively (or quantitatively) qualifying their level of welfare (or current economic situation) and, subsequently, identifying the potential BDH beneficiaries. Special attention is given to verifying the accuracy of the information provided by families during the Registro Social survey process. The household information is later cross-referenced with data from the social security administration, the public electricity company, and the national credit bureau to ensure that people with formal and stable jobs of relatively high salaries, with a consumption of energy higher than the limit set for programme eligibility, with active loans granted by private banks, or people who own a vehicle, are not prioritized when selecting programme beneficiaries (De la O, 2015).

The implementation of the surveys is carried out in three different ways depending on the number of households in each census sector and on their geographical location. In most cases, families are visited and surveyed in their own homes. In some less frequent cases, families are summoned through a local authority to go to a public place where the surveys are applied in groups. Additionally, the MIES allows families to voluntarily register their interest in becoming future programme beneficiaries. This can be done by phone, in person or in the official web page of the Registro Social (MIES, 2013).[25]

[24] The UBN methodology establishes that a household is considered poor when at least one of five different basic needs is not satisfied. These basic needs are related to housing materials, access to basic services, level of overcrowding, economic dependence and school attendance (Feres & Mancero, 2001).

[25] For more information about the Registro Social process, more specifically the methods used by the MCDS to identify census districts and collect household information, see Chapter 4, page 152.

The socioeconomic information collected through the surveys is then integrated into the Sistema de Información del Registro Social (SiiRS). The SiiRS is the database used to estimate the Welfare Index for each of the surveyed households, which fulfils the function of targeting mechanism (i.e. beneficiary selection mechanism) of the BDH programme. The Welfare Index is merely a proxy means test (PMT) that uses the statistical technique of non-linear main components, which consists of combining 34 variables to build a weighted average and, as a result, generate a household welfare proxy (MCDS, 2016). The information used to build the proxy is related, for example, to the housing characteristics, access to basic services, availability of assets, family composition, educational attainments, etc. Consequently, the Welfare Index takes values that range from 0 to 100, where 100 is the highest level of well-being.[26]

The selection criteria used to choose the potential beneficiaries of the BDH programme is based on targeting households that have a score equivalent to or less than 28.20 in the Welfare Index. This includes extremely poor households located in the first quintile of the welfare distribution (i.e. 24.07 points) plus a margin of 4 points that serves to reduce possible exclusion errors (MIES, 2013c). However, since the BDH programme implementation in 2003, the process of identifying potential beneficiaries has been modified twice –both after the socio-economic data collection by the Registro Social– that is, once in 2008 and once in 2014.

Initially, in 2003, the targeting mechanism was the PMT called SELBEN Index with its specific calculation methodology. The target population was those families that were in the first and second quintiles according to the values of the index (i.e. poor households). Later, in 2008, the methodology used to calculate the SELBEN Index was changed and the index was formally renamed as the Welfare Index, while the target population stayed the same. Finally, in 2014, both the targeting mechanism (that is, the

[26] For a more detailed explanation of the Welfare Index, its statistical technique and the variables used for its construction, see Chapter 4, page 157.

PMT methodology) and the target population were modified (Martinez et *al*., 2017).[27] The following table summarizes the most important changes in the BDH potential beneficiary identification process that should be considered when selecting the sample for the impact evaluations:

Table 1: Modifications in the BDH Beneficiary Identification Process (1998-2014)

	Bono Solidario	Bono de Desarrollo Humano (BDH)		
Year of implementation (modification)	1998	2003	(2008)	(2014)
Targeting Mechanism	Self-targeting	SELBEN Index (PMT 1st methodology)	Welfare Index (PMT 2nd methodology)	Welfare Index (PMT 3rd methodology)
Target Population	Poor households	Poor households	Poor households	Extreme poor households
Eligible Households Criteria	Non-applicable	Quintiles 1 and 2 (up to 36.5 index points)	Quintiles 1 and 2 (up to 36.5 index points)	Quintile 1 (up to 28.2 index points)
Number of Surveyed Households[28]	Not available information	2.1 million	2 million	2.1 million

Source: Own elaboration based on MCDS (2016) and Martinez et *al*. (2017).

[27] The Welfare Index with the 2008 methodology is the one used in this thesis to estimate the impact of the BDH and CDH programmes. Fortunately, in 2014, different indexes were constructed using the two available methodologies to facilitate their comparison.

[28] Note that the number of households surveyed in each Registro Social is not equivalent to the number of BDH beneficiaries.

Once the potential beneficiaries of the programme are objectively identified using the Welfare Index, the public institution in charge of the BDH proceeds to select those individuals who will effectively receive the cash transfers (MIES, 2013). Although in theory all individuals living in poverty have the same right to receive these monetary benefits, throughout the implementation of the programme not all of them have been able to be covered for strictly budgetary reasons (Gutierrez et *al.*, 2013). Therefore, according to the bases of the BDH and to the principle of equal rights among individuals, the final selection of beneficiaries should and *apparently* takes place through a strictly random process, in which all potential beneficiaries have the same probability of being chosen (Calvas, 2010; MCDS, 2013). This last statement is key to justify the validity of the natural experiment proposed in this thesis as one of the methods used to evaluate the effectiveness of the BDH programme, which is described in detail in Chapter 3. However, at this point it is necessary to mention that there are some considerable reasons to believe that the selection of BDH beneficiaries might not be as random as it seems.

In the first place is the geographical issue, that is, the territorial location of the households surveyed by the Registro Social. As mentioned above, the survey process focuses mainly on the census sectors with the highest incidence of poverty (i.e. percentage of poor households over 50 percent). Therefore, those individuals or families in extreme poverty who are not located in the pre-selected sectors are arguably less likely to be visited, surveyed and subsequently chosen to receive BDH cash transfers. Note that in order for these families to be able to be selected, they must first personally request the visit of the MIES staff within the established deadlines (MIES, 2013). Second, there are political factors or interests that could in some way affect the beneficiary selection process. Ultimately, the decision to grant transfers to a particular household or group of households could be largely political and, therefore, is likely to be subject to value judgments, electoral interests, circumstantial issues and even internal negotiations (McCord et *al.*, 2016). It is possible, for example, that the government prefers to benefit certain social sectors that are more organized or that have greater political representation.

Finally, there are socio-economic and cultural factors that could *indirectly* affect the chances of a household in extreme poverty being chosen to receive BDH cash transfers. The levels of education, income and access to technology (and information) play an important role when households need to exercise the right to social assistance and acknowledge a broad range of other social rights (such as the rights to education, health, social security, leave, employment and pensions) (Sepúlveda, 2014).[29] In the case of the BDH programme, the potential beneficiaries are responsible for updating their data and verifying if they have been chosen at the end of the selection process (Martinez et *al.*, 2017). Therefore, illiteracy, long distances, limited access to basic public services and Internet service, and the lack of an adequate 'technological culture' are some of the factors that could arguably reduce for some people the possibility of being a beneficiary. Although some of these socio-economic factors are observable, there are others that are not, raising serious doubts about the theoretically random selection of beneficiaries.

Unfortunately, there is no adequate and specific review of the aforementioned points in the existing literature on the BDH. The only argument mentioned quite frequently is that all individuals in extreme poverty have the same right to be chosen to receive the benefits of the BDH programme (Calvas, 2010; Gutierrez et *al.*, 2013; MCDS, 2013). However, it is necessary to take into account the existence of geographical, political and socio-economic factors that clearly raise the possibility that the process of selecting beneficiaries is not *completely* random. In other words, there could be internal selection biases (i.e. the presence of observable and non-observable differences between groups) that must be taken into account when evaluating the programme, that is, when comparing BDH beneficiaries with non-beneficiaries (as will be explained thoroughly in Chapter 3). In any case, whether the selection process of BDH beneficiaries has been completely random or not, the evidence shows that this process has been sufficiently efficient so that the programme's targeting results have been quite successful over time (Gutierrez, 2016).

[29] In Ecuador, as in other Latin American countries, the incorporation of a rights approach in the design of its integrated social protection system is the outcome of the legal and institutional development required by its constitution (Sepúlveda, 2014).

It is said that a government is successful in its targeting effort if all or most of the benefits reach those who need them most. This would mean that the programme is *progressive* in the sense that its subsidies (or transfers) are concentrated mainly on the target population. The higher the percentage of benefits reaching the target population, the more progressive is the programme. On the contrary, the government's effort is not successful if a significant portion of the target population is not benefitting (i.e. under-coverage problems), or there is a significant loss or leakage of subsidies (or transfers) to other groups (i.e. leakage problems). The latter would also mean that the programme is regressive (Bitran & Munoz, 2000).

Some evidence suggests that the BDH programme has been *progressive* since its implementation, given that cash transfers have been concentrated mainly on the target population (i.e. first all those in poverty and then only those in extreme poverty). Specifically, until 2013, close to 30 percent of the benefits reached the extremely poor households (1st quintile), while close to 35 percent were concentrated in the moderate poor households (2nd quintile) (Gutierrez et *al*., 2013). Evidently, the changes in eligibility conditions that occurred in 2014 modified this distribution and somewhat improved the progressivity of the BDH programme. In 2016, only the households in the first quintile represented about 42 percent of the total beneficiary households (Gutierrez, 2016).

The existing evidence also indicates that there have been small but persistent filtering and sub-coverage problems, which were not completely solved despite the changes in the eligibility conditions of the beneficiaries. In fact, these changes considerably increased the sub-coverage level of households living in extreme poverty. More specifically, the coverage of households in the first quintile went from 63 percent in 2013 to 41 percent in 2016 (Gutierrez, 2016). Therefore, in the last few years, very high levels of exclusion of potentially eligible households are estimated. This has meant that Ecuador has gone from being one of the countries in the region with a CCT programme with the lowest level of sub-coverage (of households in extreme poverty), to have sub-coverage levels close to the regional average and much higher than those of Chile, Bolivia, Uruguay and Mexico.

On the other hand, in terms of leakage problems, there is a significant reduction after 2014 –without this important issue being completely solved. Specifically, it was estimated that, in 2013, close to 35 percent of benefits reached the population above moderate poverty level (2nd quintile) (Gutierrez et *al*., 2013). While, according to more recent estimations, "only" 20 percent of the benefits reached families above the second quintile in 2016 (Gutierrez, 2016). Undoubtedly, there is a great need to develop more sophisticated targeting strategies so that CCT programmes, such as the BDH, can effectively reach their target populations and solve the persistent filtering and sub-coverage problems. This is an important challenge not only for Ecuador, but for other countries in the region that use similar targeting mechanisms. The seeming progressive nature of the BDH programme makes us think that there should not be serious leakage problems in the sample used in this thesis for the impact evaluations. In other words, those who belong to the BDH treatment group should actually be poor. Of course, this will be confirmed later in Chapter 5.

2.2.3 Structure and Payment Mechanisms of the Cash Transfers

Unlike other CCT programmes, there is no registration process for the BDH in which the selected beneficiaries must present some documentation in order to receive the payments. Basically, once the MCDS identifies the potential beneficiaries, this information is sent to the MIES which, as the institution responsible for executing the programme, applies the selection criteria, prepares a database of selected households and activates the payments. Moreover, there is no direct mechanism of notification (e.g. email or postal service), since it is the families who are responsible for finding out if they were chosen. However, massive radio and television campaigns are carried out by the government to inform families of the mechanisms to verify if they were selected (i.e. call centre, Registro Social website and the MIES local offices).

The structure of the BDH cash transfers is the same as that of most CCTs in Latin America, that is, it consists of a payment of 50 US dollars delivered every month of the year.[30] Initially, the programme assisted poor households with 100,000 Ecuadorian Sucres (equivalent at that time to 15 US dollars), later in 2009 the cash transfer was fixed at 35 US dollars, and finally in January 2013 it was increased to 50 US dollars (see Table 2). The monthly payments are administered mainly by the National Banking System (BANRED) together with other private financial institutions that also provide transactional services but with less relevant participation (MIES, 2013).

Table 2: Changes in the Monetary Value of the BDH

Year	Cash Transfers Amount (in dollars)
1998	$15.10
2000	$11.50
2003	$15.00
2007	$30.00
2009	$35.00
2013	$50.00

Source: MIES (2013).

One of the most important characteristics of the BDH payments is that the programme presents a clear gender perspective in the delivery of benefits by providing cash transfers primarily to the woman of the household (in approximately 99 percent of households). This perspective is based on the belief that the woman usually has the greatest responsibility for purchasing decisions, preparation of food, health care of

[30] This amount represents about 13 percent of the total monthly income for a family in the first quintile of the Welfare Index (which is around 366 US dollars).

children and monitoring the attendance of children to school (Martinez & Rosero, 2007). The idea that children do better when their mothers control a larger fraction of family resources is supported on a vast literature on the dynamics of intra-household allocation of benefits (Lundberg et *al*., 1997).

It is important to note that in addition to the conditional cash transfers, families receive no other complementary intervention to help them overcome poverty and improve their living conditions, such as nutritional assistance and talks, nutritional supplements or any other kind of incentives to change the usual behaviour of the beneficiary families. However, any family in extreme poverty can participate in other MIES cash transfer programmes apart from the BDH. These programmes include a pension for elders and a pension for people with disabilities, which also provide families with 50 US dollars a month. In these cases, the person receiving the benefits is the elderly or the member of the household with disability and not necessarily the woman (MIES, 2013).

Regarding the payment mechanisms of the transfers, the BDH is one of the few programmes in the region that do not issue electronic payments in a systematic manner (for example, using a debit card issued by a bank) and in which the beneficiary families do not necessarily require a bank account.[31] For that reason, the BDH programme developed a wide network of payment points both in urban and rural areas, which include private networks of non-bank correspondents and several public and private financial institutions. In December 2015, the BDH payment network had 9,740 payment points. (Martinez et *al*., 2017).

Another alternative, although much less used, to collect cash transfers is through ATMs. In order to do so, beneficiaries must request a magnetic card called MIES Bono Rápido, which is not linked to a bank account and which allows ATMs to identify the BDH beneficiaries. However, this initiative, which ultimately seeks the modernization of

[31] However, when BDH beneficiaries apply for the CDH programme, it is necessary that they have a savings account to receive the transfer. As explained in detail below, the CDH programme is a non-reimbursable credit for investments that only BDH beneficiaries can access.

the payment mechanisms, has not had the expected reception in the beneficiaries of social assistance programmes (Martinez et *al*., 2017). This may have been mainly due to the fact that the majority of beneficiaries are not used to employ this type of financial instruments, added to the lack of knowledge and the lack of ATMs, especially in rural areas. According to the MIES official information, 593,000 people requested the card when it was implemented in 2013, but only about 10,000 cards were still active 4 years later. It is important to note that the initial high demand of the magnetic card shows that there is actually a lot of interest in the use of this alternative, but the technological limitations have prevented this initiative from replacing the collection in payment points as the preferred payment mechanism.

Finally, the BDH beneficiaries also have the option of receiving the cash transfers directly in their savings accounts. This payment mechanism was recently implemented to facilitate transactions. Despite the notable advantages of using this alternative, especially in relation to the high transaction costs assumed by the government, there are also some disadvantages that are worth mentioning. In the first place, the beneficiaries need to open an account and assume the costs of managing, which are normally charged by the banking entities. In addition, only the payment points (or ATMs) that belong to the financial institution that issues the account can be used for collecting the money without incurring an additional cost.

2.2.4 Conditionality of the Programme

In theory, a cash transfer scheme is conditional on education and health if minimum thresholds of school attendance and health check-ups are required and there are well-established protocols for verifying conditionalities (De la O, 2015). Since its implementation, the BDH was conceived in accordance with its operational rules as a programme of conditional benefits involving two types of conditioning.

First, the programme establishes a series of conditions related to the health of the beneficiary families. If the household has children under 5 years old, the transfer is conditional on children going to health check-ups at least twice a year. Therefore, mothers of beneficiary families must present a vaccination card every month to ensure that they are taking their children to medical controls in order to prevent health problems and malnutrition. In the case of pregnant women and children under one year of age, they must complete five or six health consultations per year. In addition, members of the household of childbearing age must attend an annual talk on family planning (Martinez et *al*., 2017). Table 3 summarizes the series of health-related conditions that are theoretically imposed by the BDH programme.

Table 3: Heath-related Conditions of the BDH Programme According to the Member of the Household and the Age of the Children.

Member of the household	Necessary health check-ups per year
Pregnant women	2 check-ups in the first four-month period
	2 check-ups in the second four-month period
	1 check-up at the end of pregnancy
Children under 1 year of age	2 consultations (0 to 4 months of age)
	2 consultations (5 to 8 months of age)
	2 consultations (9 to 12 months of age)
Children between 1 and 5 years of age	Minimum 2 medical consultations
People of childbearing age	Attend a talk on family planning

Source: Martinez et *al*. (2017).

In the case of non-compliance with the previously described health-related conditions, the BDH programme establishes clear sanctions with progressive severity. That is, after the first breach of one of the conditions, the beneficiary family receives only a formal warning. The second breach implies a reduction of 20 percent of the value of the cash transfer, while the third causes a considerable reduction of 40 percent. After the fourth breach, the payment should be permanently suspended according to rules established by the MIES.

Second, the BDH programme also establishes conditions related to the education of the children. If the beneficiary household has children between 5-18 years old, the transfer is conditional upon all children being enrolled in a general basic education school or high-school[32], and attending at least 80 percent of school days in accordance with the academic calendar. Thus, in order to receive the transfer, mothers must submit an original certificate indicating that their children are enrolled and regularly attending their schools, or instead they must submit the official grades/report card of their children (MIES, 2014).

As with the health-related conditions, the MIES formally established sanctions for non-compliance with the conditionalities related to children's education of the beneficiary households. Specifically, according to the rules of the BDH programme, a child not formally enrolled in school implies a 50 percent reduction of the cash transfer, while a child not regularly attending classes entails the complete suspension of the payment.

However, in practice, some operational challenges and budgetary constraints – combined with high turnover among the programme's technical staff– prevented for many years the efficient verification of compliance with conditionalities (Martinez & Rosero, 2007). In the early stages of the programme, government officials claimed that it was not financially possible to impose the requirements because the administrative burden and costs of monitoring school and health facilities attendance are very high (Ponce &

[32] In Ecuador, general basic education comprises ten school grades: a first grade of preparatory education for children of 5 years of age, three grades of elementary education, three grades of intermediate education and three grades of secondary education. Once the general basic education is completed, the children goes to the baccalaureate or high-school that comprises other three grades.

Enriquez, 2013). In fact, verifying that conditionalities are met is usually a complex and expensive process that involves staff at the programme's head office, active participation from schools and health centres, and a municipal associate, who is typically the representative for the programme in the local government (De la O, 2015). As a result, for about ten years (i.e. from 2003 to 2011), no follow up measures or monitoring tools were implemented to ensure compliance with the requirements and co-responsibilities demanded by the BDH programme.[33]

It was only at the beginning of this decade that the MIES and MCDS reported that they have begun to randomly monitor the compliance among beneficiary families (MIES, 2013b). Specifically, a partially systematic process has been implemented (and different pilot tests performed) to monitor compliance with the conditionalities, although there have been no officially reported suspensions of payment to those beneficiaries who do not comply with them (Martinez et *al*., 2017). Nevertheless, although there is no complete systematic process for the verification of the formal conditions, the BDH beneficiaries have been continually informed about the "co-responsibilities" or conditionalities that must be met through communication campaigns.

Interestingly, despite the fact that no requirement was monitored or enforced for many years, according to the perception of a substantial portion of potential beneficiaries, the BDH was a conditional programme from the beginning (Oosterbeek et *al*., 2008).[34] In other words, there has been a common perception that transfers have been conditioned in a large proportion of the target population. This could be explained by the first indications that programme officials gave families during the registration processes and also because of the chain of communication carried out by the programme at the beginning of the BDH.

[33] Different evaluation reports conducted in the previous decade indicate that the BDH programme had no formal mechanisms to verify attendance at school and health care centres (Paxson & Shady, 2007; Schady & Araujo, 2006).

[34] Recent surveys conducted by Oosterbeek et *al*. (2008) indicate that at least one third of the beneficiaries believed that the transfers were conditional since the programme's implementation in 2003.

Actually, evaluation reports have pointed out that a possible reason for this common belief among programme beneficiaries is that before the actual implementation of the BDH programme –specifically at the time that people were enrolling to receive cash transfers– there was a massive media campaign, which stressed the importance of schooling and health care. As part of this campaign in the media, programme administrators explicitly stated on radio and television that parents are responsible for school attendance and health of their children and discussed how this is linked to the benefits of BDH (Paxson & Shady, 2007; Schady & Araujo, 2006). In consequence, most households believed that there were enrolment requirements that were going to be monitored after the implementation of the programme or in the near future.

Finally, it is important to point out that the health- and education-related conditions that BDH beneficiaries must meet –and the consequences of their non-compliance– were first established in 2003 along with the implementation of the programme, but then they were drastically modified in 2013. In the past, health controls did not adjust to children's age or the progress of pregnancy, nor were there requirements related to family planning. Nowadays, the conditions attached to the programme are much stricter than those initially established (Martinez et *al*., 2017).

The MCDS recently implemented more conditionalities related to the housing conditions of beneficiary households, child labour and child begging. In this sense, families are required not to building their houses in areas prone to flooding or landslides, and the invasion of land is strictly prohibited. Besides, they must keep their houses and their neighbourhoods in acceptable hygiene conditions. Moreover, parents are committed to the programme not to send children under 15 years to work or beg. Formally, the penalty for non-compliance with any of these conditions is the temporary or definitive suspension of the cash transfers.

Given that the BDH is (at least in theory) a conditional programme and that the beneficiaries have the perception that non-compliance with these conditions can lead to the withdrawal of benefits, one would expect the results of the impact evaluations –in

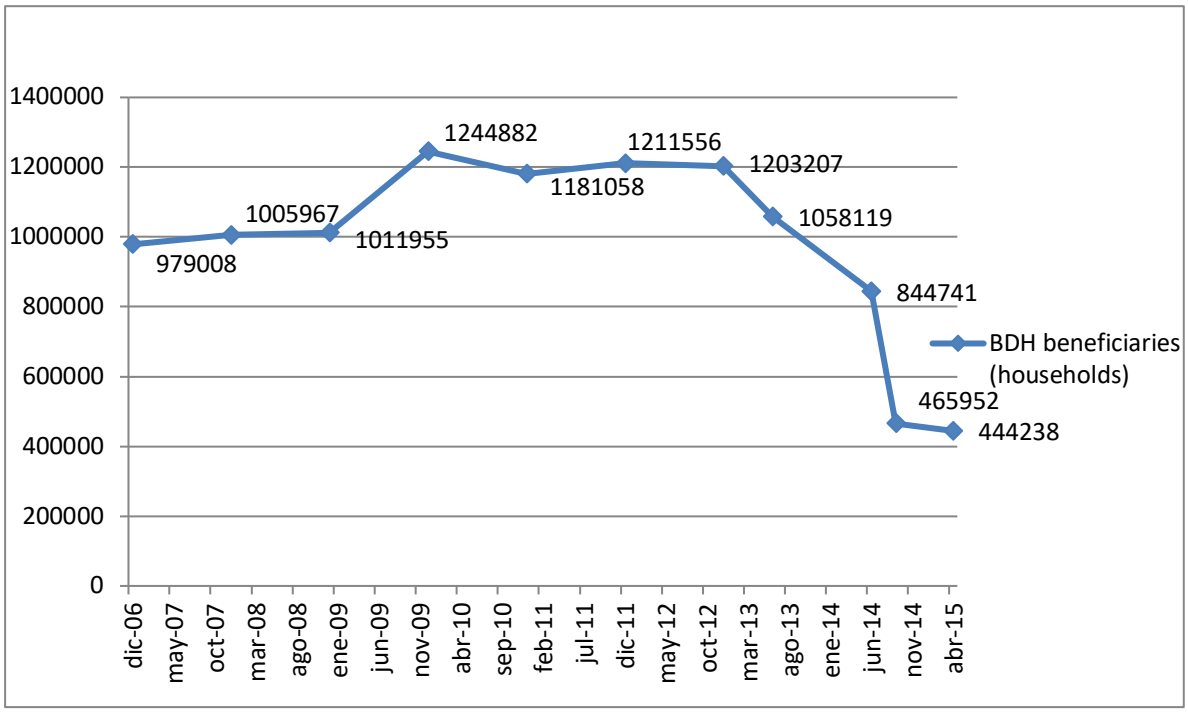
terms of human capital generation– to be positive. Otherwise, it would mean that the programme is not reaching one of its main objectives and would raise serious doubts about whether the MIES is effectively verifying and controlling compliance with the conditions. Moreover, it is necessary to consider that changes in conditionalities, making them much stricter, began only one year before the last Registro Social survey, so that their effects on individuals' behaviour still would not be reflected.

2.2.5 Coverage of the Programme

The number of beneficiary households of the BDH programme increased significantly and almost continuously from 2006 to 2010, as shown in Figure 2. The relatively constant upward trend was consolidated since 2008 mainly due to the approval of the new Ecuadorian Constitution, which gives priority in terms of rights and attention to vulnerable groups of the population. As stated in Article 35 of the Constitution, people in situations of vulnerability or poverty, the elderly, children, pregnant women, persons with disabilities, persons deprived of liberty and those suffering from terminal illnesses must receive priority attention and specialized care in the public and private sectors (Constitution of the Republic of Ecuador, 2008).

From 2011 until 2013, a slight decrease can be observed in the level of coverage that responds mainly to the political decision to reduce public spending on social assistance programmes (Gutierrez et *al*., 2013). Figure 2 shows, for example, that from December 2012 to June 2013 there was a reduction of 145,088 beneficiary families. However, until that time the BDH programme still covered a significant portion of the Ecuadorian population (i.e. around 1.1 million households) (MIES, 2013b).

Figure 2: Beneficiary Families of the BDH at the National Level



Source: Own elaboration based on MIES (2013b) and Martinez et *al*. (2017).

The most significant reduction in the number of beneficiaries happened as of 2014. This decrease is mainly due to the new system that, according to the MIES, has been used in recent years to improve the targeting mechanisms of the programme and to debug the database of beneficiaries by solving persistent leakage problems (MIES, 2013b).[35] From this process of re-targeting and adjustment of the eligibility criteria, a large number of families have left the programme. In total, 748,000 households that received the BDH were excluded between April 2013 and April 2015 (Martinez et *al*., 2017).

[35] Since 2013, the SELBEN database is cross-referenced with data from the social security administration, the public electricity company, and the National Bureau of Credit.

2.3 The Crédito de Desarrollo Humano Programme

Arguably the most innovative welfare programme to date in Ecuador, which comprises the more significant variations with respect to traditional CCTs, was announced in 2007 and fully launched at the end of 2008. This programme was called the Crédito de Desarrollo Humano (Human Development Credit) and it consists of an unconditional lump-sum payment (equivalent to twelve BDH cash transfers) to households living in poverty who ask for it voluntarily in order to invest the money in productive ventures. In this sense, the Crédito de Desarrollo Humano, commonly known as CDH, could be defined as a *non-reimbursable* productive microcredit designed for those who are willing to promote projects that favour productive or self-employment activities (Martinez et *al*., 2017).

The CDH has its predecessor in the Crédito Productivo Solidario (CPS) programme implemented in 2001 and also aimed at households receiving BDH transfers. This programme was based on the idea that the poorest households would overcome poverty with the support of the government, but mostly because of their own efforts (Castillo et *al*., 2017). The government provided small loans of up to 840 US dollars to a maximum term of up to 2 years and those in poverty contributed with their productive capacity to improve their income (Registro Oficial, 2001). Therefore, the CPS was a reimbursable microcredit not linked to the BDH cash transfers, interest rates were agreed based on market conditions and payment guarantees were requested.[36] However, as should have been expected, this programme did not reach the desired levels of coverage and acceptance among vulnerable groups of the population, due in large part to the high interest rates in the national financial market, and also to the fact that poor people have many difficulties to provide a guarantee of payment.

[36] See <http://http://www.inclusion.gob.ec/credito-productivo-solidario/> for a detailed explanation of the requirements to access the CPS programme (02/04/2018).

After this failed attempt to promote inclusive microcredit, it could be said that the lesson was learned and a totally new programme was designed with terms and conditions of credit much more suitable for vulnerable populations. Actually, the only formal requirement of the CDH programme is to be a beneficiary or at least a potential beneficiary of the BDH programme at the time of application. Thus, people who participate in this alternative programme have the advantage of accumulating the BDH cash transfers for a full year and use this capital to start or strengthen their own businesses. The usual payment is up to 600 US dollars (one year of accumulated monthly transfers), personal guarantees are not required, and -unlike traditional microcredits- any kind of repayment is necessary (MIES, 2013b).[37] At the moment when a potential beneficiary family requests the CDH micro-credit, the MIES simply processes the payment of the accumulated transfers and deposits it in a bank account.

The main purpose of the CDH programme is to improve the quality of life among the poorest families in the country by providing access to financial products that strengthen and promote their economic capacity, while boosting the national economy through the efficient allocation of capital to productive investments (Maldonado & Moreno, 2011; MIES, 2013b). Moreover, the CDH programme aims to facilitate access to knowledge through expert advice, technical support and links to other existing social services. According to the Instituto de Economía Popular y Solidaria (IEPS), the CDH programme was designed to meet the following specific objectives: create opportunities for local development, expand the market for micro and small businesses, encourage the creation of micro joint venture projects, improve credit policies in the country and ensure the sustainability of micro productive enterprises (IEPS, 2013, page 2). In order to meet these objectives, the CDH is based on three fundamental axes: productive inclusion, associativity and sustainability.

Productive inclusion is sought by promoting the generation of autonomous income and the increased social participation of the poor populations, while *associativity* is

[37] In some cases, a small annual interest rate (normally 5 percent) is retained at the time of disbursement.

fostered by financing collective and self-managed work opportunities (IEPS, 2013). Although most families who apply for the CDH alternative already run small businesses and microenterprises -engaged in the production of goods, trade or services- there is also the possibility of applying for the CDH programme through the elaboration of a simple business proposal or a productive investment project, which has to be previously assessed and approved by the programme administrators. Thus, previous business experience is desirable but not necessary to be eligible to participate in this innovative scheme of cash transfers (IEPS, 2014).

Moreover, business *sustainability* is sought by accompanying the ventures with training and assistance in different topics to ensure that they remain profitable over time (IEPS, 2013). It has been established previously that –apart from financial products– a true productive inclusion requires an enabling economic and social environment that offers opportunities for those in poverty (see page 16). For this reason, the CDH programme accompanies the lump-sum cash transfers of a series of complementary policies (related to effective micro-entrepreneurship and personal development) that seek to improve the capacities of beneficiaries and break down the structural barriers that they commonly face. The continuous support of the Ecuadorian government to the CDH micro-entrepreneur, through technical advice and capacity-building activities, is thought to be important for families to start their own businesses and be successful.

At this point, it is useful to clearly identify the expected medium- and long-term results of the CDH programme and the pre-conditions necessary (according to the Ecuadorian Government) for it to be effective. This information will be used in the evaluation design process since it allows to understand the workings of the programme, as well as the causal effect relations between inputs and outputs. The theoretical-approach to the impact evaluation is made in a later chapter through the description of the CDH Theories of Change (ToC) (see page 270). The IEPS (2014) indicates that the expected results of the programme are the following:

- Declines in income poverty at national and local level, specifically in the areas where there is a higher concentration of CDH beneficiaries.
- Increased partnership activities aimed at the development and improvement of production in different industrial and strategic sectors.
- Significant reductions in unemployment and underemployment throughout the country.
- Improvements in welfare and living standards of the most vulnerable population.

Moreover, the MCDS (2010) in its *Implementation Guide of the CDH Training Plan* formally identified the series of activities that were required for the correct implementation and subsequent development of the CDH programme. First, a comprehensive and timely record of the potential beneficiaries of the programme was generated. This registry of people living in poverty is periodically updated by the Registro Social system. Second, the programme was widely socialized through the media and people were encouraged to consider working in partnership as a valid form of productive enterprise. Third, the creation of micro-enterprises was promoted not only as a measure of subsistence, but also as a way of personal development. Specifically, the government used different success stories in productive investments, setting them as an example for the general population of how entrepreneurship can be translated into individual and social progress. Finally, trained personnel were hired to provide technical support to the beneficiary families, identifying problems and proposing solutions. However, it is important to keep in mind that these pre-conditions may have been necessary, but by no means are sufficient to ensure that the CDH programme has been successful.

2.3.1 Target Population and Targeting Mechanism

As previously mentioned, the CDH programme is in some way linked to the BDH to the extent that only the potential beneficiaries of conditional monthly transfers can access the productive microcredits. This means that both the target population and the targeting mechanism are *initially* the same for the two programmes. More specifically, the accumulated CDH cash transfers are aimed at families around the first quintile of the Welfare Index (i.e. households that have a score equivalent to or less than 28.20 index points); while the process to identify these potential beneficiaries is through the socio-economic information of the Registro Social.[38] Therefore, in order to grant the microcredits, the same eligibility criteria are used as for the BDH cash transfers.

However, it is important to emphasize for the subsequent impact evaluation of the CDH programme in this thesis that, once the families are already identified as potential beneficiaries of the BDH and CDH programmes, the only possible targeting mechanism of the CDH programme is through *self-selection* (or self-targeting) as there are no additional formal requirements. In other words, unlike the BDH programme, the final selection of CDH beneficiaries is not random. It is the families themselves who ultimately decide to opt for the lump-sum payment instead of the monthly transfers (MIES, 2013b).

2.3.2 Structure and Payment Mechanisms of the Lump-Sum Cash Transfers

The CDH cumulative (or lump-sum) cash transfers are structured as periodic payments, equivalent to twelve regular BDH cash transfers, that give those in poverty the possibility of developing a productive process while linking to the formal economy. Thus, this

[38] Recall that until 2014 potential BDH and CDH beneficiaries also included families around second quintile of the Welfare Index (i.e. households that have a score of up to 36.5 index points).

innovative form of cash transfers can be also seen as annual (non-reimbursable) micro-credits (MIES, 2013b).

The application process of the CDH programme is simple and is carried out in the first instance by telephone and then in person. Specifically, households interested in requesting the microcredit must first contact the MIES call centre to inquire about the availability of credits and apply for the programme. By telephone, it is verified that the household is enrolled in the BDH programme or at least qualifies as a potential beneficiary. In addition, it must be confirmed that the household does not have access restrictions due, for example, to the non-fulfilment of any of the conditionalities that have been registered in the past. Once the telephone operator verifies compliance with these basic requirements using a computer system, a personal appointment is scheduled at the MIES agency that is most convenient for the applicant (Martinez et *al*., 2017).

The key element for the efficient operation of the application process for CDH lump-sum cash transfers is precisely the MIES integrated computer system, which allows managing the demand for microcredits and facilitates the attention to the applicants in a timely manner. This system collects and integrates information from different databases and from different sources. For example, the Banco Nacional de Fomento and several private credit cooperatives continually inform MIES about the number of people that can be attended and the number of credits that can be issued in each agency, according to the availability of resources in each financial institution.

There is only one payment mechanism of the CDH cash transfers and it requires the opening of a bank account in case the applicant does not have one at the moment of the application. Normally, all financial institutions and credit cooperatives charge a certain amount for the opening of the required savings account, which must be covered in full by the applicant. Once this last requirement has been met, the applicant must attend the agreed appointment and present the necessary documentation that includes the personal and banking information of the household. Then, the MIES operator proceeds to the approval of the CDH microcredit and its deposit through the beneficiary's savings account.

In general, the payment process, understood as what happens between the approval of the credit and the deposit of the money in the savings account, takes two business days (Martínez, 2014).

The CDH micro-credits are thought to fulfil a very important mission considering that access to credit by vulnerable groups (i.e. the poor, people with disabilities and older adults) in Ecuador has traditionally been constrained by private banks. There are more than 50 financial institutions in the country (cooperatives, NGOs and specialized banks) that provide microfinance products, but the majority of Ecuadorians do not easily access these services. According to the National Network of Development Financial Institutions, 60 percent of people in the country have their own businesses, but 92 percent of them do not have access to credit, so they have to start their ventures with their own capital or with an informal loan (Zumba, 2017).

There are a number of reasons that cause the institutionalized exclusion to financial products/services of those in poverty. Some of them are related to the lack of guarantees, the considerable financial expenses, the lack of external funding, the internal cost of reaching remote locations, the apparently unattractive financial returns, and the long distances between potential customers. In consequence, inequalities in access to credit have been accentuated over time and have diminished the opportunities that these population segments require to enter the productive sectors and improve their living conditions. Undoubtedly, a key short-term policy challenge in Ecuador is to design more specialized financial products that cover the credit needs of these populations that, although generally have few economic resources, in volume represent an attractive market. In this sense, the CDH programme might play an important economic and social role, attending a market segment that private banks have left aside for many years.

The CDH programme offers four different alternatives or types of credit, which differ in the amount of the transfer, the type of recipient, and the purpose of the credit. The most popular type of CDH transfer is the *individual* credit that is granted to finance self-employment activities or new ventures dedicated to production, commerce or

services. Its maximum amount is 600 US dollars and the interest rate withheld is 5 percent. Another type of CDH is the *business* credit, which is granted to already established businesses. Its maximum amount is 840 US dollars and the interest rate retained is the same as the individual one.

In third place is the *articulated* CDH credit whose purpose is to help and encourage beneficiaries to join a collective project or programme managed by the IEPS. Its maximum amount is 900 US dollars (1 year and a half of accumulated transfers) and the interest rate is maintained. Finally, the *associative* CDH credit is granted to those collective productive organizations registered in the IEPS and it is accompanied by specific training that promotes associativity and strengthens group dynamics. Its maximum amount is 1200 US dollars (2 years of transfers) and the interest rate is 5 percent.

Table 4: Amounts and Terms of Each Type of CDH

	Individual	Business	Articulated	Associative
Amount	600 US dollars	840 US dollars	900 US dollars	1200 US dollars
Interest Rate	5 percent	5 percent	5 percent	5 percent
Term	12 months	12 months	18 months	24 months
Amount delivered to beneficiary	578.57 US dollars	798 US dollars	854.29 US dollars	1,140 US dollars

Source: IEPS (2014).

Although in the impact evaluation of the CDH programme conducted as part of this research project it will not be possible to distinguish between the type of credit granted to each beneficiary, it seems important to make this distinction so that the reader understands the level of customization/adjustability of the CDH initiative. By developing productive inclusion strategies tailored to each beneficiary of social assistance, the

programme manages to cover a greater number of unmet needs, which are different for each individual. Undoubtedly, this is a desirable feature that the new generation of integrated social assistance programmes are taking into account.

2.3.3 Complementary Policies/Practices

In order to achieve the productive inclusion of those in poverty, the CDH programme combines cash transfers with different public sector policies. Perhaps the most distinctive and innovative design feature of the CDH is that it offers free-of-charge technical support and assistance in capacity-building to the new entrepreneurs as an integral part of the programme (MIES, 2017). These complementary interventions are intended to guide and develop the productive capacities of the beneficiary households. More specifically, they are implemented with the intention that beneficiaries acquire some of the general knowledge and basic skills needed to start and run successfully their own small businesses. Therefore, the CDH programme –apart from providing cash transfers– seeks to promote the formation of human capital and minimize the financial and social risks to which beneficiaries are exposed (i.e. loss of BDH social protection benefits and destabilization of consumption over time).

Expert technicians from the Economic Inclusion Unit of the MIES continuously monitor the CDH micro-enterprises in different productive sectors (e.g. agricultural, textile, industrial). They are in charge of helping beneficiaries to solve problems and of promoting partnership between them as a desirable business strategy. Attending different training workshops (e.g. strategic partnering and performance consulting), CDH beneficiaries gain knowledge about the effective management of resources for handling savings, investments, interest, operational costs and household expenses.[39] According to the MIES (2017), these practices have allowed an important segment of the population,

[39] See <http://www.siise.gob.ec/siiseweb/PageWebs/Accion%20Social/ficacc_OS035.htm> Integrated System of Social Indicators for a detailed explanation of the programme's more distinctive features (15/04/2015).

which was traditionally excluded from the financial market, to access complementary services and use their money in productive initiatives that subsequently allow them to improve their income levels, decrease dependence on social assistance and improve their quality of life.

In addition to providing CDH entrepreneurs with knowledge in business management, the MIES professional advisors also help them develop certain individual skills to be successful in their personal life and in the business world: flexibility, dynamism, creativity and persistence. These skills play a very important role since the entrepreneurship projects made by people living in poverty face all kinds of difficulties, relatively superior to those of any other venture, because they are made by people in conditions of vulnerability. The professional assistance offered by the government seeks to promote the social inclusion of people with learning, so that they are able to adapt to changing and unfavourable circumstances, minimizing the risks of their investments (Martínez & Mariño, 2013).

However, according to some researchers, it is still necessary to strengthen these complementary policies/practices so that the ventures are truly successful and the beneficiaries get more out of them. Martínez and Mariño (2013) conducted surveys to CDH beneficiaries in the city of Latacunga to find out their perception about the programme and the use they have made of the transfers. Regarding the complementary services, the majority of beneficiaries indicated that they only received one visit per year from the MIES, which was aimed to help them start the business. The authors maintain that, although it is true that the MIES (through the Institute of Popular and Solidarity Economy) monitors CDH beneficiaries for two years, the visits are not frequent enough and it is necessary to improve training on certain technical aspects (such as, market management and strategic planning for business growth).

For this reason, these researchers recommend increasing the number of visits and counselling sessions for programme beneficiaries, as well as, more support in the commercialization of their products. In addition to the assistance provided to those in

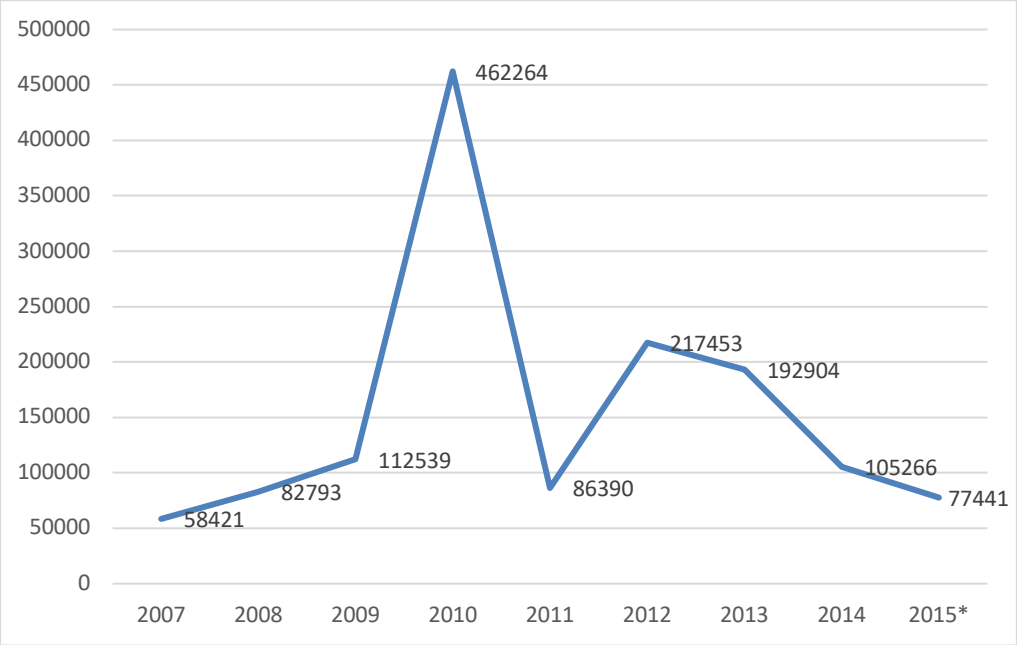
poverty, better monitoring and training practices for beneficiaries would be an alternative way of verifying the appropriate use of the transfers. Moreover, they suggest the implementation of evaluation processes that allow entrepreneurs to know their weaknesses and strengths, which could help them make better business decisions in the future. In any case, despite the great efforts made by the government, it is undeniable that there is still much to be done in relation to these complementary policies (Martínez & Mariño, 2013).

2.3.4 Coverage of the Programme

As can be seen in Figure 3, the number of CDH credits granted increased annually since the implementation of the programme in 2007 until 2010. In 2007, 19.9 million US dollars were allocated to the programme and 58,421 CDH transfers were delivered. One year later, in 2008, there was a significant growth in the volume of transfers (41.72 percent) and 28.09 million US dollars were allocated to the programme. In 2009, there was a growth of 42.63 percent in the number of credits requested with respect to the previous year and 43.93 million US dollars were placed (MCDS, 2015).

The year 2010 is the fastest growing period of the CDH, as the number of credits increased by 391 percent and 360 million dollars were allocated. Note that, while in 2007 approximately 58,000 credit operations were carried out, in 2010 this figure was eight times higher. In fact, the CDH alone became the fourth largest participant in the national microcredit market in that year (IEPS, 2014). This growing trend goes hand in hand with the coverage levels of the BDH programme that also increased progressively until that same year.

Figure 3: Number of Annual Credits Granted by the CDH



Source: Own elaboration based on MCDS (2015).
*CDH credits granted until September 2015.

However, the number of productive microcredits granted by the CDH was considerably reduced as of 2011. This decline was mainly due to the fact that several of the credits delivered in 2010 were requested at 24 months and that there was a significant reduction in public spending on social assistance programmes that has been maintained since that year (see Appendix A). In 2012, the coverage level of the CDH programme had an important rebound, but it only lasted for two years because of the changes in the eligibility criteria for welfare programmes (see page 74).

In the period between 2007 and 2015, the CDH programme provided around 1.4 million productive microcredits (MCDS, 2015). This represents an average of 155,000 credits per year, which means that a significant percentage of BDH beneficiaries applied for the CDH credit instead of the monthly transfers, especially after the changes in the eligibility conditions as of 2014. However, it is important to note that the number of

households that have benefited from the programme is lower than the number of credits granted, since many of them requested CDH transfers more than once. Currently, the volume of microcredits delivered by the CDH programme exceeds 100,000 credits per year.

Between 2007 and 2012, the CDH microcredits were allocated mainly to households in the urban area, where 746,651 transfers (72.8 percent) were recorded. On the other hand, in the rural area only 278,697 transfers (27.2 percent) were made during the same period. In monetary terms, during the first 6 years of implementation, 424,64 million US dollars were allocated for the urban area and only 154,73 million US dollars for the rural area. Similarly, five provinces account for 62 percent of the total transfers along this period: Guayas (29 percent), Manabí (13 percent), Los Rios (9 percent), Pichincha (6 percent), and Esmeraldas (5 percent). Four of these provinces belong to the coastal region, while only one of them belongs to the Andean region (IEPS, 2014).

According to the IEPS (2014) figures, the main economic activities to which CDH transfers were allocated are: commerce (65.1 percent), agriculture and fishing (31.1 percent), manufacturing (3.4 percent), services (0.3 percent), construction (0.1 percent). Most of these transfers were requested by beneficiaries (and potential beneficiaries) of the BDH (89.4 percent). While the elderly and people with disabilities requested 10.6 percent of the credits, which shows that some of them are economically active.

Moreover, the type of CDH credit with the highest participation is the Individual CDH with a total of 872,133 transfers, representing 85.1 percent of the total amount of transfers. Since 2008, the Business CDH transfers have been registered with a total of 143,283 credits, which represent 14 percent of the total. Finally, the modalities of Associative and Articulated CDH account for approximately 1 percent of transfers during this period of analysis. However, in 2013, the Associative CDH had an interesting upturn compared to previous years. Specifically, 19,904 associative credits were delivered nationwide, which represented an investment of more than 22 million US dollars. With these cash transfers, 771 productive projects were generated (IEPS, 2014).

2.3.5 The Local Policy Debate on the Crédito de Desarrollo Humano

Government officials have persistently argued over the last years that the CDH programme has had good results in building capacities and opportunities to overcome poverty in a sustainable manner (MIES, 2013b).[40] More specifically, through different institutional reports, the Ecuadorian government states that the CDH programme helps those in poverty to access preferential credit and use it in productive initiatives that allow them to improve their income, decrease their dependence on social benefits and subsidies and, consequently, improve their quality of life (IEPS, 2014).

These assertions have been supported by perception studies such as that of Martínez and Mariño (2013), which indicates that 56.7 percent of the beneficiaries consider that the CDH programme substantially improved their living standards. Additionally, in terms of revenue, 64.9 percent said they had somewhat improved their income with their new microenterprises, while 35.1 percent said they had made very significant income improvements. Remarkably, this study indicates that *all* the beneficiaries surveyed claimed to have used their credits to start a new venture or expand their current business. Most of them focused their ventures in the agricultural and livestock sector (which are the main economic activities in the rural area of the province of Chimborazo). Specifically, 79.7 percent used the CDH to invest in businesses dedicated to animal husbandry and agriculture. However, it is also important to note that the majority of respondents consider the amount of the CDH transfer insufficient for the creation of a productive microenterprise. As a result, they have had to partner with other beneficiaries to work together, which represents a clear example of a socially desirable outcome obtained perhaps in an inappropriate and involuntary way.

[40] See <http://www.inclusion.gob.ec/?s=credito+de+desarrollo+humano> Ministerio de Inclusion Economica y Social (23/03/2015).

Similarly, Coba and Diaz (2014) made visits to a sample of 261 beneficiaries of the CDH in the province of Tungurahua to study their economic activities, their level of satisfaction and how much they improved their income. Through the use of observation forms, they found that 74.3 percent of the credits have been invested in livestock activities, 13.8 percent in manufacturing activities, 0.4 percent in footwear raw materials, 0.4 percent in rabbit breeding and the rest in other activities such as the trade. In addition, the authors indicate that 156 microenterprises (60 percent) have had excellent or good results (additional average monthly income of 85 US dollars or more) and 44 microenterprises (17 percent) have had regular results (additional average income between 50 and 85 US dollars). However, 23 percent of the projects show poor results, because these beneficiaries have additional income that does not exceed the value of 50 US dollars per month equivalent to the BDH transfers. The economic activities that obtained the highest average income were those related to footwear raw materials, pork production, agriculture and manufacturing. Interestingly, 97 percent of the projects were carried out by women.

Although there is some empirical evidence to support the government's arguments in favour of the CDH, some academics and politicians (mainly from right-wing opposition parties) have instead argued against the implementation of this programme. Generally, typical arguments are repeated against the delivery of cash transfers to the poor (see page 21) or in favour of conditionalities (see page 41). They have claimed, for instance, that poor people are not capable enough to handle lump-sum transfers and even less to manage their own businesses. Besides, from the beginning, one of the biggest concerns has been that it may be that a significant part of the money is not actually being used for investments in productive activities; and that this programme may be incentivizing those in poverty to work less and depend on the government. In fact, it is probable that many of the credits were destined for family consumption, which was in some way justified and even promoted mainly in 2008, when the financial crisis in the developed countries began to have consequences in the Ecuadorian economy and generated a contraction in domestic demand, which caused a small recession afterwards (Bermeo, 2013).

Therefore, the majority of criticisms made to the CDH programme have been based on the paternalistic and welfare dependency perspectives on cash transfers. These arguments are supported by studies such as Castillo, Salazar and Espinoza (2017), who analyse the creation of new microenterprises through CDH cash transfers. The data was collected through visits made by university students to 153 beneficiaries in the province of Guayas. Among the main results, they found that 30 percent used the entire CDH credit for entrepreneurship, 32 percent used it partially, and 38 percent did not use the credit to start a business but in personal expenses. That is, a good part (around half) of the CDH transfers was not invested in productive activities. Among the people who did start businesses, most did selling clothes, meals, or setting up small stores.

In addition to the theoretical arguments against the implementation of the CDH, there are also some practical concerns about its operability and efficiency: i) there are still weaknesses and lack of clarity in the guidelines of the credit, ii) there is no evaluation process for credit assignment, iii) beneficiaries need more and better training in order to undertake sustainable productive activities, and iv) now that the accumulated cash transfers are delivered one (or even two) years in advance, there are no possible mechanisms to control and enforce children's attendance in school and in health care centres (Maldonado & Moreno, 2011). Therefore, contrary to what programme administrators claim, it has been strongly suggested in academic and political spheres that social outcomes may not have improved in terms of overall well-being, human capital formation and productivity, which are the most important targets of social assistance programmes.

According to Ponce (2013), for example, the CDH programme completely changes the meaning of traditional CCTs (such as the BDH), that is, the way in which social assistance practices are commonly understood in the developing countries.[41] In fact, he suggests that the CDH is actually a microcredit programme and stops working as a

[41] Although Ponce (2013) considers this a criticism of CDH transfers, it could actually be one of the most important contributions of the programme.

human capital generation programme.[42] More specifically, given that the CDH leaves aside conditionalities in health and education, it is no longer necessary for beneficiaries to send their children to school/health-centres to receive the money, which could lead to a decrease in school enrolment/health-status among those in poverty. Additionally, since the CDH consists in the advance delivery of a transfer, there would be no practical way to enforce that the money is being used in productive ventures (i.e. absence of control mechanisms). Finally, even if the beneficiaries productively invest the transfers, the businesses undertaken by people living in poverty are normally so precarious that they could promote child labour.

As can be seen, there is some empirical evidence that supports both the arguments in favour and the arguments against the implementation of this innovative programme. Nevertheless, the CDH has received great government support and its acceptance among those in poverty has grown fast (MCDS, 2015). In fact, as previously discussed, it reached very high levels of coverage only a few years after its launch and its share of social assistance spending has increased significantly over time. The main problem that the Ecuadorian government has faced, when it comes to justifying spending on this unconditional programme and obtaining the necessary political support for its continuation, has been the lack of conclusive evidence about its efficiency (i.e. its real effect on different social outcomes).

There are currently no significant impact evaluations on the CDH, mainly due to the lack of experimental data. It is not very clear if this was due to the ethical considerations involved in carrying out an experiment of this type, or to a planning error on the part of the institutions responsible for implementing the programme. One could think that it was a combination of both reasons; however, only a few months ago it was announced that a project called "Impact Evaluation of the CDH", presented by the MIES, was one of the 6 winners of the First International Call for Impact Evaluations for Better

[42] Ponce's (2013) criticism refers to the unconditional nature of the CDH, but omits, among other things, the fact that the complementary CDH policies (e.g., training and technical assistance) seek to develop the individual capacities of the beneficiaries.

Public Management convened in 2017 by the Corporacion Andina de Fomento (CAF)– Development Bank of Latin America.

Leaving aside any ethical considerations, this project consists of experimentally evaluating the impact of a pilot programme, which will introduce reforms to the current CDH programme, on indicators related to the level of well-being (labour income, welfare index, living conditions, employment status, consumption patterns) and on outcome variables related to the productivity of the enterprises (time of operation, number of employees, level of investment, etc.). For this to be possible, the CAF will provide technical support to the MIES throughout the evaluation process. The main objective of this future study is to provide rigorous evidence on the effects of the CDH programme (CAF, 2018).

It is also important to mention that for several years it was not even considered the option of conducting a quasi-experimental evaluation of the CDH, mainly due to the lack of qualified personnel to carry out this kind of study in most Ecuadorian public institutions. This has changed only 3 years ago when, as a result of training provided by the Eurosocial Programme of the European Commission, the only existing impact evaluation of the CDH was carried out using one of the quasi-experimental methods proposed in this thesis.[43]

The Eurosocial technical advice took place in March 2015, when a training workshop on quantitative methods of impact evaluation for the analysis of public policies was carried out. Thirty civil servants participated in this workshop, both from the Secretaría Nacional de Planificación y Desarrollo (National Secretariat for Planning and Development) and from the Ministerio de Finanzas (Ministry of Finance). The final aim of this cooperation activity was to provide the methodological tools for the effective

[43] Eurosocial is a cooperation programme between Latin America and the European Union which seeks to contribute to improving social cohesion, as well as to institutional strengthening through support to their processes for design, reform and implementation of public policies (Eurosocial, 2018).

evaluation of different strategic programmes in Ecuador, including the CDH programme (Eurosocial, 2018).

To the best of my knowledge, the results of this evaluation have not been officially published. However, thanks to the support of the MCDS –institution with which I had a direct communication throughout the development of this thesis and those who have provided me with the data used in it– I had access to the final report of the Eurosocial training with respect to the CDH, as well as to the constructed database used for its evaluation. Using a difference-in-differences strategy, they found a positive effect of the CDH on the Welfare Index. In fact, it was also shown that the intensity of CDH treatment plays an important role in determining the magnitude of the effects of this intervention on the level of well-being among individuals in poverty. More specifically, at higher intensity (i.e. more CDH credits granted to a given household), the positive impact is greater.

The main limitation of the government's evaluation is that it only takes into account a very small sample of individuals and its national representativeness is questionable. On the other hand, the main advantage of this evaluation is that it considers the intensity of treatment – through the integration of data from the MIES registers on the number of credits granted to each person. Given that the government's impact evaluation of the CDH conducted in 2016 uses one of the quasi-experimental methods (i.e. diff-in-diff) and one of the outcome variables (i.e. Welfare Index) that I proposed (around three years before) to the MCDS and the Secretaria Nacional de Estudios Superiores Ciencia y Tecnología (SENESCYT) (National Secretary of Superior Studies Science and Technology) as part of my research project, I would like to think that this thesis has already positively influenced Ecuadorian policy-makers to understand the importance of promoting the culture of impact evaluation in this area and the possibility of employing alternative quantitative methods in the absence of experimental data.[44]

[44] This research project was presented to the Ecuadorian government for approval and funding (through the SENESCYT scholarship programme) at the end of 2013.

Thus, it will be a core contribution of this thesis to assess and compare the impact that the two most important social assistance programmes in Ecuador have had on overall well-being, human capital investment and labour supply of beneficiary families in recent years– and also evaluate how the design of more integrated cash transfer initiatives could contribute to poverty eradication in Ecuador, and possibly elsewhere. For this purpose, I propose the use of different quasi-experimental methods together with two specific impact evaluation designs, contributing to an evaluative literature where experimental methods have predominated.

CHAPTER 3
Methodological Alternatives for Evaluating Cash Transfer Programmes

3.1 Introduction

The empirical evaluation of the impacts of cash transfers represents an important methodological challenge. Generally speaking, the allocation of monetary transfers (as well as in-kind transfers, or free access to goods and services) across households is (almost) never random, but is typically based on socio-economic characteristics such as wealth, living conditions, individual effort, employment status, or other selective mechanisms. Hence, the same individual features that determine the probability of receiving any kind of cash transfers are perhaps correlated with the outcomes of interest. Since some of these individual features are unobservable, this correlation creates a *selection problem* that normally prevents the accurate estimation of the effects of welfare programmes such as monetary transfers to poor families (Rawlings & Rubio, 2005; Arnold et *al*., 2011).

In other words, due to the common absence of random assignment of individuals to treatment and control groups in cash transfers initiatives, simple comparisons of pre-

treatment and post-treatment outcomes are likely to be contaminated by individual characteristics (or events) affecting the outcomes under study, since both might be correlated (or occurring at the same time). This recurrent impact evaluation difficulty is normally referred to as the selection problem. For instance, the human capital outcomes (education and health) of individuals participating in a cash transfer programme cannot just be compared with those who did not participate in the programme if the selection of beneficiaries is *not* random, but rather based on reported income levels. The reason is that it is possible that the results are mostly driven by the same specific characteristics used as a mechanism of selection (such as income in the previous example), because these characteristics may be correlated with human capital as well. As a consequence, differences between treatment and control groups might be due to factors other than the cash transfer programme itself (Brewer & Picus, 2014).

The following sections of my methodological chapter address this selection problem in the impact evaluation of cash transfers by exploiting first a natural experiment and then two different quasi-experimental designs. In general, these types of research designs are applied in a wide variety of studies that resemble randomized controlled trials (RCTs), but that lack the researcher control or the random assignment characteristic of a true experiment (McKenna & Morrison, 2009). Moreover, natural- and quasi-experiments come in many forms, including before-after comparisons, group-to-group comparisons, or a combination of both known as difference-in-differences (Angrist & Pischke, 2009).

According to Remler and Ryzin (2010), there are a number of reasons why natural- and quasi-experiments are important and widely used in social research literature. First, it is often not possible (or correct) to carry out randomized experiments in social and policy research due to practical or ethical constraints (i.e. absence of experimental data). Second, quasi-experimental studies can be conducted on a larger scale or in more realistic environments than RCTs, improving their generalizability and their relevance for policy or management decision-making. Finally, the evaluator can perform quasi-experimental studies more easily and with a lower budget.

In view of these important advantages offered by quasi-experimental methods, it is at least surprising that the majority of impact evaluations of cash transfer programmes in developing countries are carried out through RCTs that –in addition to being costly and difficult to implement– raise considerable ethical and methodological concerns (Devereux et al., 2013). The very idea of randomized assignment is antithetical to social protection programmes because it means that a group of poor people who genuinely need assistance (i.e. the control group) will intentionally be deprived of cash transfers, even when they have the right to receive government support. Besides, it has been argued that although RCTs may be robust in terms of 'internal validity' (i.e. the extent to which a causal conclusion is warranted), they are weak in terms of 'external validity' (i.e. the extent to which the results can be generalized and replicated) (Ravallion, 2009).

Given that the experimental evaluations are commonly carried out and financed by the same international organizations that promote the implementation of conditional cash transfer (CCT) programmes, it is likely that their popularity will be explained by the same reasons why conditionality has become entrenched in social protection in the developing world (i.e. greater concern for political support and funding sources than for ethical considerations). The evaluation of cash transfer programmes is generally carried out by the multilateral institutions that finance them and, although the reasons are arguably not clear, it is a fact that institutions such as the World Bank or the BID have preferred the application of experimental methods for this purpose. A possible logical explanation could be the lack of secondary data or efficient information systems, but this has not always been the case.

Of course, it is important to note that although natural- and quasi-experiments have many well-known advantages, they also have significant shortcomings. The main one is that usually these studies exhibit less robustness than random experiments in terms of demonstrating causation (i.e. establishing a cause-effect relationship) (Dinardo, 2008). All the same, they are still very popular because of their versatility, and also because -like most quantitative research designs- their fundamental purpose is to estimate the causal impact of an intervention on the outcomes under study using inferential statistics to

generalize findings from a sample to a population (Remler & Ryzin, 2010). These alternative methodological approaches, along with their implicit assumptions and an overall assessment of their validity, are discussed in more detail below as they will be used to evaluate the impact of the BDH and CDH programmes on different outcome variables.

3.2 Natural Experimental Design

Natural experiments are observational studies which help to overcome some of the common difficulties researchers face when assessing the outcomes and impacts of policy interventions. They are typically situations which can be exploited to better answer a research question, rather than experiments that researchers have influenced or designed by themselves (Dunning, 2008).

Therefore, unlike in randomized controlled trials, researchers do not have the possibility to assign subjects to the 'treatment' and 'control' groups in advance. However, noticeable divergences in law, policy or practice offer the opportunity to study populations *just like* they had been part of a randomized or true experiment (McKenna & Morrison, 2009). Essentially, a *non-controlled* portion of the population is affected by an intervention or event, while the other is not. This event is considered "natural" in the sense that it was not planned. As will be acknowledged later in this chapter, the validity of any natural experiment depends on the key assumption that the assignment of treatments to participants is random, or more precisely, "as if" random.

According to Sekhon and Titiunik (2012), these naturally occurring experiments share some important characteristics with RCTs. Nevertheless, the authors argue that there are fundamental differences between the two types of designs that give rise to conceptual and inferential problems, which are usually overlooked in the existing research literature. In a natural experiment, the mechanism that assigns treatments to subjects is not known with certainty to be random. Rather, an unplanned event occurs in the world that affects

some individuals but not others, and the researcher assumes –supported by some theoretical or empirical evidence- that the treatment was assigned *as-if* randomly. This assumption of as-if randomness is frequently referred to by scholars as *exogeneity* of the explanatory variable (treatment).

In most studies, researchers put a lot of effort in verifying that this assumption is satisfied. The concept of exogeneity implies that treatment assignment is not influenced by factors associated with the outcome of interest (i.e. confounders). Thus, the treatment and control groups, created by the experiment in a "natural" way, should be similar in terms of all observed and unobserved characteristics that may somehow affect the outcome, with the clear exception of the treatment and confounding factors controlled by the researcher in the estimation model. Only when the two groups are alike in this way, is it possible to consider that the design is valid and, thereby, the actual treatment effect could be properly estimated (Angrist & Pischke, 2009).

Moreover, natural experiments differ from randomized controlled trials in another fundamental way, which in turn is the cause of their most difficult problems to overcome. Since the naturally occurring intervention or event affects some subjects but not others, it is usually possible to define several different treatment and control groups. However, only some of these groups are similar in the way described above, and therefore, valid to be compared. The problem is that in most cases it is not immediately obvious which groups are comparable, leading researchers to frequently compare the wrong groups (Sekhon & Titiunik, 2012).

In other words, the artificially constructed treatment and control groups sometimes may not be comparable, even when it is assumed that the treatment allocation was random. Additionally, even if we are confident that the design is valid (because the chosen groups have similar characteristics), this comparison may not estimate the true causal effect of the intervention, but the effect of an omitted variable or an event that coincidentally happened at the same time. Although to be fair, this last issue is not exclusive to natural

experiments, but common to all quasi-experimental research designs (as will be discussed later in this chapter).

At this point it is important to mention that, despite the well-established shortcomings described above, natural experiments have become a major component of scientific research in recent years. In fact, they have been widely used by researchers in a broad array of topics and fields, such as public health (Friedman et *al*., 2001; Lindahl, 2002), educational psychology (Bronzaft & McCarthy, 1975), political participation (Brady & McNulty, 2004; Lassen, 2005), development economics (Kudamatsu, 2011), comparative politics (Keefer & Khemani, 2011), and social policy (Pickard, 2012). It is undeniable that among the social sciences, economics and epidemiology are the leading fields to exploit the natural experimental approach in their research, but political science and interdisciplinary fields like social policy are also well represented (Dunning, 2008). In order to get a better idea of how to recognize a natural experiment, it is useful to briefly examine some examples of the way this type of design has been used by social scientists to help make causal inferences. The examples presented below -although not directly related to the topic of this thesis- are intended to complement the previous explanation by describing two studies in which the exploited "natural" interventions arise from very different circumstances, yet in both cases treatment allocation is considered equally random.

In a recent study, Pickard (2012) was interested in assessing the relationship between formal and informal care for older people, and the impact of changes in social policy in Britain. According to the author, the time period between 1985 and 2000 provides the necessary conditions for a natural experiment. Certainly, during the late 1980s/early 1990s, there was a rapid increase in long-stay residential care, which suddenly came to an end in the mid 1990s. Pickard's article uses General Household Survey data to examine whether the increase in institutional care led to a decline in co-resident care by adult children for their older parents.

In this natural experiment, the treatment was assigned to one cohort (exposure group) and not to the other (non-exposed group) because it varies through a "naturally" occurring event (application of different policies over time) that happens to be exogenous to the outcome under study. The overall findings suggest that, between 1985 and 1995, care in nursing homes/hospitals did replace family care in many cases. Indeed, the number of people aged 80 and over in long-stay residences doubled around those years, while there was a fall by around a half in informal care by adult children. On the other hand, between 1995 and 2000, there was a significant increase in provision of family care due to changes in the community care system that affected the chances of moving to an elderly care institution.

Altogether, this study argues that, after controlling for disability and age, there is considerable evidence of substitution between formal care and co-resident care for older people. A key policy implication of these results is that an expansion of publicly funded long-term care for the elderly might lead to a significant increase in demand, which would be accompanied by the decline of informal care. It is useful to note that in this natural experiment, the researcher does not need to depend on a priori reasoning or empirical evidence to defend the exogeneity assumption, as she only takes advantage of a true randomization of the intervention generated by divergences in policies throughout time: it is an "unplanned" event what randomly assigns subjects to treatment and control groups (exposed and non-exposed groups). In other words, this natural experiment is merely a cohort study in which exposure groups are assumed to be, on average, the same as would have been obtained with true randomization.

Nonetheless, most natural experimental designs in the social sciences involve interventions considered "as if" randomly assigned to subjects, rather than treatments that are allocated by using a true randomization method. A paper by Keefer and Khemani (2011), for example, assumes random treatment assignment to examine *how* radio access improves public service outcomes in Benin. The potential effect of mass media on education and health outcomes seems somewhat obvious, as media provide important information for many people, including of course those in poverty. In fact, strong evidence

suggests that radio access improves private benefits from government welfare programmes (Stromberg, 2004; Reinikka & Svensson, 2005). Accordingly, donors have made significant efforts to set-up and support community radio stations in many less-developed countries.[45] However, before Keefer and Khemani's paper (2011), there was little rigorous evidence of the channels for such association.

It had been widely hypothesized that the effects of media on public service outcomes arise because increased information facilitate collective action of citizens to extract greater benefits from their government. Until that, using household data from Benin, Keefer and Khemani (2011) find that greater access to community radio is indeed associated with an increase in usage of education facilities; but the impact actually came from changes in household behaviour, which lead to a higher take-up of government programmes. According to the authors, the appropriate radio programming seems to persuade people to invest more into the health and education of their children.

The analysis rests on survey data of more than 4,000 households and 210 villages, spread across 32 communes in northern Benin. These villages exhibit substantial variation in access to community radio, which is driven by the limited signal strength of broadcasters and the geographical differences between villages. Thus, the necessary conditions are given to take advantage of a natural experiment in which the treatment under study (access to radio stations) varies through some unplanned or naturally occurring event that happens to be exogenous to observed and unobserved village characteristics.

In particular, Keefer and Khemani (2011) find significant evidence that the media effect does not operate through government responsiveness: "Schools in villages with greater access to community radio exhibit no differences with respect to either government inputs (such as books and teachers) or the efficiency with which inputs are utilized (such

[45] Unlike other types of media, community radio promotes the importance of human capital and prioritizes the broadcast of public-interest programming on a wide range of subjects, such as democratic values and human rights.

as lower teacher absenteeism)" (p. 7). Instead, the evidence largely supports the proposition that the impact of media on public outcomes operate through changes in parental behaviour: "Instrumenting for household listenership of community radio with village access to radio, we find that listening leads households to purchase more books and to pay tuition for children's education" (p. 8). In other words, households with greater media access are more likely to make financial investments in their children's education. Overall, these findings are consistent with both the idea that mass media plays an important role in development, and the belief that parental behaviour matters for children's participation in schools (Fraja et *al*., 2010).

3.2.1 Natural Experiments in the Ecuadorian Cash Transfers

Now, with a better understanding of the concept of natural experiment and how to apply it in research, I will focus on the first type of design presented in my thesis. Note that, in general, when we talk about cash transfers, a natural experiment occurs only when the selection (or self-selection) of beneficiary families for the programme is not based on observable and unobservable socio-economic characteristics, but it is haphazard and possibly random (just like winning the lottery). In this case, I take advantage of a natural experiment arguably occurring in the assignment of two different types of cash transfers (treatments) to some of the poorest households in Ecuador.

Since 2003, when the Bono de Desarrollo Humano (BDH) was launched, the Ecuadorian Government has made great efforts to better target the monetary transfers towards those in poverty. As discussed in the previous chapter, one major step was the development of a composite welfare index known as SELBEN. In theory, until 2014, the 40 percent of Ecuadorian families —those poorest according to the SELBEN score— were eligible for the 50 US dollars monthly transfers or the accumulated benefits offered by the Crédito de Desarrollo Humano (CDH) (see page 74). However, the limited budgets of the programmes and their operational mechanisms have been insufficient to cover all

households in the first and second poverty quintiles, so expansion of coverage by the BDH and CDH has been rather gradual and apparently based on an *as-if* random selection of beneficiaries (see page 75). This assumption of as-if randomness constitutes the basis for the natural experiment identification strategy thoroughly developed below.

I seek to identify the effects of two different cash transfer programmes (i.e. BDH and CDH) on several outcome variables exploiting a natural experiment in the allocation of benefits. In this natural experiment, like in any randomized trial, there is a control group that estimates what would have happened to the treated group in the absence of the programme, but nature or other exogenous forces (such as budget constraints) determine treatment status instead. Thus, the validity of the control group depends on treatment status being exogenous with respect to the outcomes under study. I take advantage of two important elements characterizing cash transfer programmes in Ecuador since their early stages: i) mainly due to BDH budget constraints, families around the first two quintiles of the SELBEN index have been as-if randomly selected to participate in the programme; and ii) only beneficiaries of the BDH are allowed to switch to the CDH programme. These two facts combined allow us to think that the beneficiary selection process in both programmes was exogenous to the characteristics of the eligible recipients.

In other words, the identifying assumption for my natural experimental design is that the assignment to treatment and control groups is "*as if*" random for both programmes. Like many other policy interventions that constitute the basis of credible natural experiments in the social sciences, the starting point for the evaluation of the BDH and CDH is that these programmes involve treatments that are assigned only as-if at random, rather than through an actual randomizing device. Therefore, the type of natural experiment that arguably took place in the case of the Ecuadorian cash transfers is a standard natural experiment with as-if randomization.

However, although these interventions could in principle provide the basis for plausible natural experiments, it is also possible that the allocation of transfers is actually the product of the interaction of actors in the social and political spheres. Since it is

somewhat difficult to simply assume that these interventions are independent of the characteristics of the beneficiaries, or that they are performed in a way that does not allow individuals to self-select into the treatment and control groups, it is necessary to complement the previously mentioned theoretical arguments (on which the assumption of as-if randomness in the allocation of treatments is based) with some type of empirical verification of the assumption.

Note that in this case the identifying assumption could be verified, at least partially, only by comparing each one of the treatment groups (i.e. BDH and CDH) to the control group in terms of their observable pre-intervention characteristics, which should be reasonably similar in order to support the hypothesis. Of course, this empirical verification of the key assumption will be performed in this thesis as part of the proposed natural experimental evaluations in Chapters 5 and 7. Once there are sufficient theoretical and empirical grounds to believe that treatment status is exogenous (or as-if randomly assigned), estimation of *average treatment effects* (ATEs) is straightforward. As part of this natural experiment, I will use two different econometric techniques: a simple difference-of-means test and regression model analysis.

3.2.2 Difference-of-means Test

The difference-of-means test (t-test) is used to determine if two population means are equal by comparing two independent samples (Snedecor & Cochran, 1989). A common experiment design is to have two groups (one control and one treatment) that share the same pre-treatment characteristics, and therefore, *initially* they differ only in that one of them will receive the specific treatment and the other will not. A dependent variable (or set of variables) must be chosen on which the mean of the two groups is expected to differ as a direct consequence of the treatment.

Therefore, in this type of design there will always be two variables:

- A quantitative one (dependent variable that is going to be compared)
- Another qualitative/dichotomous (independent variable)

The idea is to verify if the quantitative variable depends on the dichotomous variable, that is, to calculate the means of the continuous variable for the two groups of the qualitative variable and create a contrast to determine whether there are significant differences or not between the means of the two independent samples (Laguna, 2014).

The significance of differences between two sample means can be assessed using a two-sample *t-statistic* calculated as part of the *t-test*. Note, however, that the t-test procedure is only appropriate when the following conditions are met:

- ✓ The sampling method should be simple randomization (or probed to be as-if random).[46]
- ✓ The two samples are independent.
- ✓ The sampling distribution is approximately normal, which is generally the case if the sample size is large enough (i.e. greater than 40, without outliers).[47]

Now, the two-sample t-statistic can be simply thought of as a scaled difference between the two means, where the absolute difference is rescaled using an estimate of the variability of the means. The reference distribution for the t-statistic is the *t-distribution*, which shape varies slightly as a function of the samples size for $n < 30$, and strongly resembles the standard normal distribution. Since the t-distribution considers the size of the sample, there is a different t-statistic for each sample size, that is, for each number of *degrees of freedom*. As the sample is larger ($n > 100$), it is almost equivalent to use the t-distribution or the normal distribution. In other words, for a high number of degrees of freedom, we can approximate N (0, 1) (Laguna, 2014).

[46] There is however no requirement that the two samples should be of equal size (Evans et *al.*, 2000).
[47] It has been shown that minor departures from normality do not affect the t-test (Moore et *al.*, 2009).

The two-sample t-statistic is defined as follows,

$$t = \frac{\bar{X}_1 - \bar{X}_2}{\sigma_{\bar{X}_1 - \bar{X}_2}},$$

Where $\bar{X}_1$ and $\bar{X}_2$ are the means of the two samples, and $\sigma_{\bar{X}_1 - \bar{X}_2}$ is simply a measure of the variability of the difference between the means. More specifically, first a pooled variance estimate is calculated as the weighted average (because the two samples are of different size) of both sample variances.[48] In notation form,

$$\sigma^2_{pooled} = \frac{s_1^2(n_1 + 1) + s_2^2(n_2 - 1)}{n_1 + n_2 - 2},$$

And then,

$$\sigma_{\bar{X}_1 - \bar{X}_2} = \left(\sigma^2_{pooled}\right)^{0.5} \left[\left(\frac{1}{n_1}\right) + \left(\frac{1}{n_2}\right)\right]^{0.5}$$

Note from the previous equations that the degrees of freedom that define this two-sample t-distribution is given by

$$df = n_1 + n_2 - 2$$

3.2.3 Regression Model Analysis

The second part of my natural experimental design consists of analysing the separate effects of the BDH and CDH programmes on several outcome variables Y by estimating different versions of the following base regression model:

$$Y_i = \alpha + \beta BDH_i + \gamma CDH_i + \sigma X_i + \varepsilon_i \,, \qquad (1)$$

[48] In this case, population variances are assumed to be equal (i.e. homoscedasticity).

where Y is any of the outcomes under study, and β and γ are the parameters of interest, which capture the causal effects of *BDH* and *CDH* (dummy variables that equals 1 for the poor households receiving cash transfers from a given programme, and 0 otherwise) on the outcome under consideration. X is a vector of individual and household level characteristics, and ε is the error term.

Specifically, I estimate different versions of Equation (1) via ordinary least squares (OLS), probit or logit regression models, depending on the type of explained variable Y.[49] The outcome variables are several, and they are evidently related to the programmes' main objectives in terms of poverty reduction and human capital. They include a broad range of observable indicators such as living conditions, school enrolment, educational attainment and labour supply.

Since I am examining the independent effects of two mutually exclusive programmes (BDH and CDH), the predictor variable of interest is a categorical variable with 3 levels or groups (one control and two treatment groups), which is examined along with other independent variables that are referred to as controls.[50] In the case of categorical variables with more than 2 groups, it is necessary to construct a series of "dummy" or "indicator" variables. Dummy variables take the values 0 or 1, and I need to have enough variables that each group has a different combination, which requires $k - 1$ variables if I have k groups (i.e. 2 dummy variables in our model). Table 5 below shows that I can use two dummy variables to determine which one of the three groups (control, BDH or CDH) an observation belongs to.

[49] The outcome variables of interest could be continuous or categorical or ordinal, and different statistical models need to be used to analyse different types of explained variables.

[50] In natural experiments controls are only included to improve efficiency, but their omission would not bias the estimates of the parameters of interest (Angrist & Pischke, 2009).

Table 5: Use of Dummy Variables to Identify Different Groups

Group	Dummy 1	Dummy 2
Control	0	0
BDH	1	0
CDH	0	1

Hence in the model specified in Equation (1), α is the expected value of Y when $dummy1 = 0$ and $dummy2 = 0$, i.e. the expected value of Y in the control group. On the other hand, β represents the change in Y as $dummy1 = 1$, i.e. the difference between control group and BDH group. Finally, γ represents the change in Y as $dummy2 = 1$, i.e. the difference between control group and CDH group. Note that this base model should be estimated as a single regression equation only in the case that I want to compare each one of the treatments (i.e. BDH or CDH) to the control group. However, it is also possible to compare any pair of the three sample groups separately. In fact, the model could be decomposed into three different versions just by restricting the sample in such a way that includes only the specific groups that I want to compare. Table 6 describes how to restrict the sample depending on the desired comparison and, therefore, the type of evaluation that will be performed.

Table 6: Sample Selection for a Two-group Comparison

Groups	Sample Selection
BDH vs Control	Potential BDH beneficiaries *not* affected by other social assistance programmes
CDH vs Control	Actual CDH beneficiaries and those not affected by *any* of the two programmes at all
BDH vs CDH	Potential CDH beneficiaries[51]

It is important to note that in this case it is possible to employ a natural experimental design due to the singular features presented by cash transfer programmes in Ecuador. However, normally finding a good natural experiment is not an easy task. Actually, it is comparable to look for a needle in a haystack (Remler & Ryzinn, 2010). Moreover, in practice the premise behind all natural experiments (namely, that treatment assignment is random or "as if" random) can be difficult to be tested, so that I should try to determine the degree of randomness that can be assumed, as well as any factor that may induce bias. In fact, according to McKenna and Morrison (2009):

> As with other quasi-experimental designs, natural experiments will never unequivocally determine causation because the researcher cannot exert control over the situation. However, they provide a useful and important inferential tool and may provide help with a research question that may not be obtainable in any other way, perhaps due to practical or ethical considerations. (p. 11)

That said, I must point out that when studying particularly the effects of the BDH programme, a fundamental issue is that the selection of beneficiaries may not be as random as it seems. As explained in the previous chapter, there might be geographic, political and socio-economic factors that, to a certain extent, may affect the targeting process of the

[51] Recall that only BDH beneficiaries are entitled to self-select themselves to participate in the CDH programme.

programme, generating considerable selection biases (see page 76). In other words, there could be non-observable differences between the treatment and control groups (e.g. in terms of incidence of poverty in the territorial location of the households, political negotiation and mobilization capacities, political representation and participation, and access to public services, technology and information), which in turn might be associated with the outcomes of interest. If this were true, the assumption of exogeneity of the explanatory variable (i.e. treatment) would be violated and, therefore, the validity of the proposed natural experimental design would be questionable (Angrist & Pischke, 2009).

Moreover, when studying the effects of the CDH programme, there is a latent concern for a potential *self-selection bias* —due to the fact that ultimately poor families are the ones who decide whether they will switch programmes (i.e. self-selection among beneficiaries). It is possible that this switching decision is based on households' own motivations or interests, which are reflected in observable socio-economic factors (such as wealth, educational attainment, or health status). If this were the case, the allocation of the CDH transfers would not be entirely "as if" random (as I certainly assumed in the natural experimental design) and the exogeneity condition would not be satisfied.

In defence of the natural experiment that arguably takes place in the case of the BDH and CDH, it can be argued that in a non-controlled experiment (like this one) it is almost never known with *certainty* that the mechanism that assigns treatments to individuals is random (Sekhon and Titiunik, 2012). This is why it is necessary to assume (as in most studies of this type) –with the support of some empirical and theoretical evidence– that the treatment was assigned as-if randomly. In addition, it would be advantageous to complement the previous natural-experimental approach with some alternative estimation methods that actually take the possible selection biases and self-selection problems into account. Specifically, an instrumental variable (IV) strategy and a difference-in-differences (DID) estimation are employed and described in detail below.

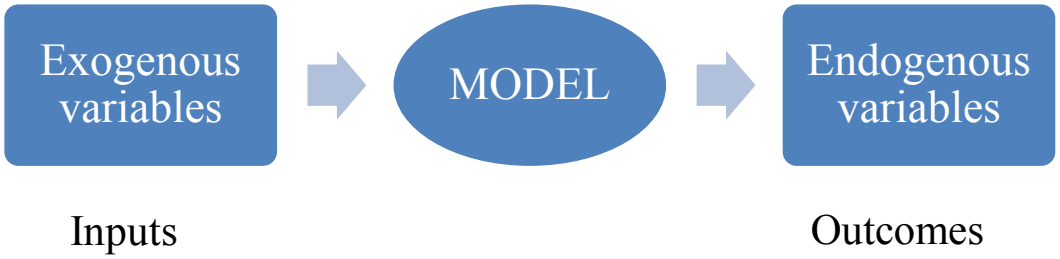
3.3 Two Alternative Quasi-Experimental Designs: Instrumental Variable and Diff-in-Diff

Before I focus on the alternative quasi-experimental designs presented in this thesis, it is useful to lay some theoretical grounds to better understand the methodological challenges inherent to this type of research. Recall that, according to the economic theory, any model deals with two kinds of variables: endogenous and exogenous (Barro, 2007). The *endogenous* variables are the ones that we want the model to explain, and therefore, their values are determined in the model. For example, the endogenous variables in the previously introduced natural experimental design include welfare level, educational attainment, school enrolment, living conditions and labour supply.

On the other hand, the *exogenous* variables are the ones considered external shocks to the system, so the model takes their values and behaviour as given (i.e. determined outside the model). Some common examples of exogenous variables include weather conditions, available technologies, price of commodities, natural phenomena and wartime situations. Note also that, throughout the first part of this chapter (i.e. the natural experiment), I have been treating the allocation of cash transfers from both programmes (BDH and CDH) as exogenous –given the assumption that treatments were assigned *as-if* randomly.

The main purpose of any model is to show how the exogenous variables affect the endogenous ones (Kanodia, 2015). As illustrated in Figure 4, exogenous variables (left box in the diagram) come from outside the model and serve as the input, whereas the endogenous variables (right box in the diagram) are determined inside the model and represent the output. Basically, the model tries to tell us how to go from one side to the other. Thus, the model may be a list of equations or graphs, or a set of conceptual ideas, which are used to predict how changes in the exogenous variables affect the endogenous variables.

Figure 4: The Workings of a Model



Now, as mentioned in the previous section, the validity of any natural experiment depends largely on the exogeneity assumption, which states that the treatment allocation to participants is considered as an exogenous variable. Therefore, if there is enough evidence supporting the idea that the CDH treatment is assigned randomly (i.e. exogenously), I could consistently and efficiently estimate the parameters of interest in Equation (1) via ordinary least squares (OLS), probit or logit regression models. However, if there are reasons to believe that the treatment status is endogenous (i.e. determined within the system); such estimates would be inappropriate and lead to inconsistent results. This is often referred to as the *endogeneity problem* (Hill et *al*., 2011).

The possible presence of endogeneity is one of the most major challenges of contemporary empirical studies in social sciences (Bech & Hillier, 2015). In statistical terms, an endogeneity problem occurs when there is a correlation between an independent variable (e.g. treatment) and the model error term (Wooldridge, 2015). The error term represents all those factors that affect the dependent variable but are not taken into account explicitly. So, in the broadest sense, an endogeneity problem arises when there is a "factor" related to the dependent variable that is also related to the independent variable under study, yet is unaccounted for in the model. Note that this factor is not necessarily a third variable –it is just "something" that is not properly taken into account.

The fact that endogeneity has such a broad meaning, suggests somehow that there are several sources of endogeneity problems in applied econometric work (Aizenman & Pinto, 2005). Actually, the three most common causes or circumstances under which endogeneity issues can arise are the following: i) omitted variable bias, that is, there is another variable that we cannot observe that affects both the independent variable and the outcome variable; ii) simultaneous causality bias, that is, the independent variable is jointly determined with the outcome variable (e.g. *CDH* causes *Y*, *Y* causes *CDH*); and iii) errors-in-variables bias, that is, the independent variable is measured with error. In addition, it is noteworthy that, since the concept of endogeneity covers different types of problems, solutions to these problems can also be quite different (Angrist & Pischke, 2009).

The omitted variable bias (OVB) is the most common illustration of endogeneity and the major difficulty to overcome with the estimation of causal effects from observational data (Anyalezu, 2015). In the simplest terms, OVB occurs when one or more important variables are left out of a model that, therefore, is incorrectly specified (Wooldridge, 2015). The bias appears in a regression analysis when the parameters are estimated, since the model compensates for the missing factors by over- or underestimating the impact of one of the independent variables. As an example, suppose I want to know if holding a postgraduate degree has an effect on wages. In order to see the correct picture, I need to take into account the effects of other factors, such as intrinsic ability. The ability of subjects may influence the likelihood of obtaining a postgraduate degree as well as the wages they are paid. If the model does not account for other important factors, such as inherent ability or individual effort, then the analysis would lead us to overestimate the impact of educational attainment.

Unfortunately, not all sources of OVB are easy to deal with. This is precisely the case when omitted variables endogeneity is caused by self-selection of subjects into the different subsamples. In fact, self-selection bias (SSB) is one of the most common threats to the internal validity of the empirical work being done in the behavioural and social sciences (Wooldridge, 2010). In technical terms, the self-selection problem arises when

subjects select themselves into a group, generating a biased sample with a non-probability sampling[52] —given that people who self-select are probably very different from the target population that the researcher attempts to analyse (Strong, 2015).[53]

Self-selection based endogeneity clearly makes determination of causality more difficult, especially in research about the effects of programmes or policies on different socio-economic groups (known as impact evaluation or outcome evaluation). For example, consider the following research question: Does private education have an impact on the academic performance of students? Initially, one could just compare the scores on standardized tests between students in private schools with those in public schools. However, it would be a mistake to assume that this simple difference reflects the "treatment" of private education. Due to self-selection, there might be a number of differences among students who choose to enrol in one or the other type of school, such as own motivation, socio-economic status, or parental influence. In other words, the decision of the students regarding where to attend school (i.e. treatment assignment) is not random, and therefore there may be differences in test scores that are not explained by the effect of private education. Consequently, it is necessary first to understand the choice (i.e. the observed and unobserved factors that lead to self-selection) before understanding the impact of that choice on the outcome of interest (Belfield & Levin, 2003).

Of course, one standard solution would be simply to control for all the observed factors that might lead to self-selection among individuals, which are certainly available through observational data. The problem with this solution is that it is unlikely to take fully into account all the important factors influencing the selection process, because some

[52] Non-probability sampling is a subjective (i.e., non-random) method of selecting units from a population. A common example of non-probability sampling is quota sampling. The problem with this method is that it is not clear whether it is possible to generalize the results from the sample to the population (Ferrell et *al.*, 2014).

[53] In a way, the self-selection bias is closely related to the non-response bias, which occurs when a significant number of individuals in the survey sample fail to respond and have relevant characteristics that differ from those who do respond (Dillman, 2000).

of the variables associated with this process are probably *unobserved*. In the words of Wooldridge (2015):

> We know that multiple regression analysis can, to some degree, alleviate the self-selection problem. Factors in the error term … that are correlated with … [the independent variable] can be included in a multiple regression equation, assuming, of course, that we can collect data on these factors. Unfortunately, in many cases, we are worried that unobserved factors are related to … [the independent variable], in which case multiple regression produces biased estimators. (p. 230)

Heckman (1979), for instance, was among the first who realized that comparing the wages of management trainees with those of non-trainees can result in biased parameter estimates, since there are unobserved characteristics of individuals (e.g., persistence, innate intelligence, effort, etc.) which may influence both the self-selection into different training programmes and future wages. Therefore, no amount of controls will address completely the endogeneity problems caused by SSB, because there are perhaps an infinite number of unobservable variables that could make the observed relationship endogenous.

This raises the need for different and more effective solutions to endogeneity in general and to self-selection bias in particular. The most popular way of dealing with these key impact evaluation problems is using quasi-experimental designs (QED) (Remler & Ryzin, 2010). Like all quantitative research designs, quasi-experiments are empirical studies that can estimate the effect of an intervention on a particular outcome of interest. They share many similarities with traditional experimental designs (i.e. randomized control trials) and natural experiments but have proven to be more useful in real-world evaluation, where the element of random assignment to treatment or control groups is not always feasible (Dinardo, 2008).

There are many different types of quasi-experimental designs -each with its own strengths and weaknesses- which have a number of applications in specific contexts (Angrist & Pischke, 2009). These designs include, among others, instrumental variables, difference-in-differences, interrupted time series, propensity score matching and

regression discontinuity. All these elaborate names are helpful to emphasize that quasi-experiments use well-defined comparison groups as an attempt to increase the internal validity of their assessments. Given that the field of QED is very large and active, I will focus only on the two types of designs that are relevant for the purposes of this thesis, namely, instrumental variables (IV) and difference-in-differences (DID) estimation.

Recall from the last paragraphs of the previous section that since I am interested in finding the effects of a cash transfer programme with unique features (i.e. CDH) –such as the self-selection of beneficiaries– I should be aware that there are compelling reasons to believe that in this case treatment assignment is endogenous (i.e. not random). More specifically, the alleged self-selection problems in the proposed natural experiment (where observed agents endogenously choose to be part of the treated group) could be considered a potential source of omitted variable bias. Therefore, in order to cope with the possible biases arising from the issues described above, I first follow an *instrumental variable* identification strategy as an alternative research design.

3.3.1 Instrumental Variable Technique

The method of instrumental variables (IV) is commonly used to estimate causal relationships in contexts in which controlled experiments are not available or when a treatment is not successfully delivered to all intended subjects in a (sometimes naturally occurring) randomized experiment (Imbens & Angrist, 1994). This type of QED is one of the most widely used approaches to address classical endogeneity problems in empirical economic analysis. In statistical terms, the IV method allows consistent parameter estimation when the explanatory variable (also referred to as covariate) is correlated with the error term of a regression model, either because of an omitted variable, measurement error, or simultaneity.[54] If knowledge, experience or statistical tests suggest the presence

[54] As already discussed, omitted-variable bias is a common endogeneity problem and may affect the results of this study due to self-selection. For further explanation on measurement error or simultaneity refer to Angrist and Pischke (2009) and Wooldridge (2015).

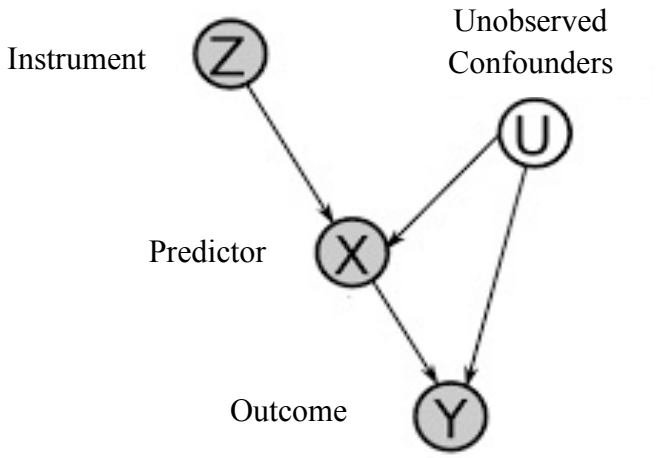
of endogeneity, ordinary linear regression usually leads to biased and inconsistent estimates (Hill et *al*., 2011). Nevertheless, if the appropriate *instrument* is found, reliable unbiased estimates can still be produced.

An instrument is a variable that causes the endogenous explanatory variable to change but does not affect the outcome in any way, other than through the same explanatory variable (Remler & Ryzin, 2010). In this sense, instruments do not themselves belong in the main equation (i.e. are exogenous sources of variation) and they are used to explain the independent variables suspected of being endogenous. The earliest known introduction to the concept of instrumental variables (as a solution to the identification problem in econometrics) appears in the appendix of a book written by Philip G. Wright, *The Tariff on Animal and Vegetable Oils*, published in 1928.[55] The biggest breakthrough of the author was demonstrating for the first time that an instrumental variable can be used to estimate the coefficient on a regressor when we are not able to rule out the presence of endogeneity.

In technical terms, if I want to get the causal effect of an endogenous predictor X on an outcome Y, I need to look for an instrument Z that is correlated with X (known as the relevance condition) and also exogenous to Y —or uncorrelated with the error term ε (known as the exogeneity condition). The first condition requires that there is some association between the instrumental variable and the regressor being instrumented, whereas the second condition excludes the instrument from being an explanatory variable in the model (Cameron & Trivedi, 2005). If this relationship holds, then Z is said to be a *valid instrument* and the IV estimator will be consistent. Figure 5 illustrates graphically both instrumental variable conditions in a directed acyclic graph (DAG).

[55] For a comprehensive review of the history of instrumental variable methodology, see Stock and Trebbi (2003), "Retrospectives: Who Invented Instrumental Variable Regression?"

Figure 5: Directed Acyclic Graph (DAG) of Instrumental Variable Conditions.



In mathematical notation, the basic reasoning to understand the consistency of the instrumental variable (IV) *estimator* is as follows.

Consider the basic relationship between output variable Y and regressor X:

$$Y_i = \beta_0 + \beta_1 X_i + \varepsilon_i$$

Since there are reasons to believe that X_i is endogenous, it is not possible to trust in the OLS estimate of β_1. However, I can still get an IV estimation of the parameters of the model as:

$$b_1^{IV} = \frac{Cov(Y,Z)}{Cov(X,Z)} = \frac{Cov(\beta_0 + \beta_1 X + \varepsilon, Z)}{Cov(X,Z)} = \frac{Cov(X,Z)\beta_1 + Cov(\varepsilon,Z)}{Cov(X,Z)}$$

If Z is a valid instrument, then $Cov(\varepsilon, Z) = 0$, and therefore:

$$b_1^{IV} = \frac{Cov(X,Z)\beta_1 + Cov(\varepsilon,Z)}{Cov(X,Z)} = \frac{Cov(X,Z)\beta_1}{Cov(X,Z)} = \beta$$

Thus, the instrumental variable estimate of β_1 will be unbiased and consistent.

The intuition behind this method is that by using valid instruments, which are orthogonal (i.e. uncorrelated) to a structural error component in a regression model, and exploiting this orthogonality as moment conditions for slope parameters of interest, it is possible to reliably estimate the slope parameters for endogenous regressors (Angrist & Pischke, 2009). Note here that this standard solution to endogeneity problems is suitable *only if* the researcher can convincingly argue that the employed instrumental variables fully satisfy the aforementioned conditions of relevance and exogeneity —based perhaps on common sense or economic theory. Consequently, the selection of appropriate instruments represents a crucial element for the credibility of the study.[56]

The instrumental variable approach is best illustrated with an example. The relationship between fertility and labour supply has been subject of very active research for decades. Empirical analyses provide substantive evidence of a negative correlation between number of children and female labour participation (Nieuwenhuis, 2014). However, the existing correlation does not necessarily imply that having more children affects negatively women's labour market attachment because other variables may affect both fertility and labour supply (i.e. omitted variable bias), or because women with low labour market attachment decide to have more children precisely for that reason (i.e. reverse causality). This self-selection into larger families among women who have lower inclination to work could create a spurious negative correlation between fertility and employment (Karbownik and Myck, 2012). Therefore, it seems clear that fertility is not exogenously determined, but rather endogenous to the labour supply of an individual.

Since it is impossible to conduct controlled experiments about fertility in the general population, empirical research aims to solve the self-selection problem by exploiting some exogenous source of variation in family-size decisions. Angrist and Evans (1998) estimate the effect of fertility on labour supply, using the preferences of American

[56] One of the best ways to determine a valid instrumental variable is to say aloud: "Z only affects Y through its effect on X"—but substitute the variables names for X, Y, and Z. For example, "Cigarette taxes only affect birth weight through their effect on maternal smoking." (Remler & Ryzin, 2010)

families for mixed-sex siblings as an instrument.[57] The authors look at women who have at least two children and estimate the marginal effect of having a third child on women´s labour supply. They use child's gender to construct an instrument based on the sex composition of siblings. This same-sex instrument is a binary variable equal to zero, if the first two children of an individual are of different sex, one boy and one girl. While it is equal to one if the first two children are of the same sex, two boys or two girls. In notation terms,

$$Z = \begin{cases} 1 \ if \ first \ two \ children \ are \ of \ same \ sex \\ 0 \ otherwise \end{cases}$$

Angrist and Evans (1998) claim that their instrument is valid because it is virtually randomized by nature and independent of mothers' characteristics.[58] The choice of the instrument also explains the reason why the authors restrict their data to individuals with at least two children. Overall, the estimation results for all women as well as married women indicate a negative causal effect from fertility to labour supply. American women who have a third child, because the first and second born are same sex siblings, decrease their labour supply by approximately 20-30 percent.[59] These IV estimates can be interpreted as a local average treatment effect (LATE) or as an average treatment effect for the American women with at least two children who comply with the instrument. However, note that there are also women in the sample who have a third child irrespective to the sibling sex composition and others who would never have an additional child -and

[57] Data comes from the Census PUMS in 1980. There are 355,356 observations in the sample, of which 302,730 women are married.

[58] The authors address all assumptions that are needed in order to estimate a local average treatment effect and provide some evidence. Yet, as is always the case with empirical work, their assumptions can be challenged.

[59] Moreover, the results also suggest that there is no impact of having a third child on the labour supply of husbands.

the estimated results cannot easily be applied to these groups of women. Thus, broadly speaking, the generalizability of the IV estimates is reduced and often unclear.

It is useful to employ the previous example to explain some of the limitations of the IV treatment effect estimation. In particular, the instrumental variable approach neither identifies an average effect on the treated (ATET) nor does it identify an average effect for the whole population (ATE). An average effect on the treated (ATET) would describe the average causal effect of having a third child on labour supply for those women who actually have a third child. However, families with three children can be subdivided into two groups, always-takers and compliers. Compliers are the ones who have a third child because the first and second born are same sex siblings. The ATET, hence, is a weighted sum of the average effect of both groups. To be precise, the weights would be the shares of always-takers or compliers in the whole population of families with three children (Angrist and Pischke, 2009). Yet, the ATET cannot be normally identified by the instrumental variable approach. Although the local average effect is estimated, the average effect on always-takers remains unknown.

A similar argument holds for the average effect on the non-treated (ATENT). In this setting, this would be the causal effect for families with two children. The non-treated can be distinguished into never-takers and compliers. Compliers are the ones who have no additional child, because the first and second born are mixed-sex siblings. Hence, the weighted sum of the average effect for never-takers and compliers cannot be determined, because the instrumental variable approach does not reveal the average effect for never-takers.

Finally, the average treatment effect (ATE) would be given by a weighted sum of the average effect on the treated and the non-treated. Again, the weights are equal to the share of treated and non-treated in the whole sample. Consequently, the instrumental variable approach does not allow any prediction on the average treatment effect, because neither of both average effects are identified (Angrist and Pischke, 2009). Therefore, the IV estimate presented by Angrist and Evans (1998) is a local average treatment effect

(LATE) which does not allow making any prediction on a randomly chosen family in the whole population.

Now, in order to explain the specific IV estimation proposed in this study, it is useful to start by characterizing identification strategies from a general perspective. After that, I will focus on the instruments, implicit assumptions and econometric technique that will be used to explore the effects of the CDH cash transfers on the outcomes of interest. Following Angrist and Kruger (1999), the term identification strategy is used to describe the manner in which researchers employ observational data (i.e., data not generated by a randomized trial) to approximate real experiments. Any appropriate identification strategy consists of two parts: a clearly labelled source of identifying variation in a causal variable; and a particular econometric procedure to exploit this information.

I start by addressing the first part (i.e. source of variation in treatment status). As described above, when assessing the impact of CDH cash transfers on socio-economic outcomes, the major difficulty is the potential endogenous-treatment issue (see pages 129-131). To overcome this problem and eventually obtain an unbiased estimate, I need to find a source of sufficient variation in the decision to opt for the CDH (explanatory variable), but independent of the poverty and human capital outcomes under study (explained variables). This implicitly exogenous source of variation will be the instrumental variable that I use for inducement into the "treatment". According to the instrumental variable conditions, in order for a variable Z to serve as a valid instrument for CDH-treatment status, the following must be true: i) the instrument must be exogenous, that is, uncorrelated with the error in the structural equation ($Cov(Z, \varepsilon) = 0$); and ii) the instrument must be correlated with the self-selection decisions of agents embodied in the endogenous explanatory variable X ($Cov(Z, X) \neq 0$).

Since it is likely that being a CDH beneficiary comprises both a random component (because only the randomly selected BDH beneficiaries are eligible) as well as a self-selection component (because ultimately the switching decision is made by families themselves), it will be necessary to restrict the sample to only those treated by

any kind of cash transfer (i.e. BDH and CDH treatment groups) in order to identify the causal effects of making the decision to change to the CDH programme.

In this thesis, CDH treatment status is instrumented using an indicator of "previous credit experience" (constructed by dichotomizing the "access to credit" variable, say 1 if any member of the household received cash loans or commercial credit in the twelve months before the 2008 survey wave, and 0 otherwise). In notation terms the instrumental variable is given as,

$$Z = \begin{cases} 1 \;\; if \;\; any \; member \; of \; the \; household \; had \; access \; to \; credit \\ 0 \;\; otherwise \end{cases}$$

The instrumental variable approach exploits exogenous variation in CDH-treatment status in order to identify the causal effect. An additional variable is therefore added to the empirical model, the instrument *Z*. Since the instrument is a binary variable, then X_{0i} and X_{1i} denote the potential treatment assignments, where X_{0i} captures the switching decision of household i for $Z_i = 0$, and X_{1i} shows the potential decision if $Z_i = 1$, respectively. Hence, the observed CDH-treatment status of an individual can be written in a potential treatment framework (Angrist, 2004):

$$X_i = X_{0i} + (X_{1i} - X_{0i})Z_i$$

The underlying assumption will be, therefore, that those families with any kind of credit experience before 2008 are more likely to prefer the CDH programme anytime between 2009 and 2014, apparently without that this past credit involvement *alone* having had a significant *direct* effect on their current socio-economic conditions. It seems logical that families who have received some credit in the past will be more willing to receive accumulated cash transfers, since they might have a greater knowledge about productive

investments. Moreover, it is possible to assume there is no direct effect on the recent outcomes because households received these credits around 2008 and even at that time they were in the first quintiles of the poverty index. Besides, access to credit before 2008 doesn't seem like an important determinant of current poverty status or human capital compared with other individual characteristics such as the level of education. In other words, previous credit participation might affect household's decision of switching programmes, without having any direct impact on the outcome variables under study. This main hypothesis can be decomposed into several assumptions that have to be made in order to allow for a consistent estimation of the causal effect. First of all, the instrument has to be 'as good as randomly assigned'. Formally,

$$\{Y_{0i}, Y_{1i}, X_{0i}, X_{1i}\} \perp Z_i$$

This assumption requires the instrument to be independent from potential treatment and potential outcome. Furthermore, the instrument has to fulfil the exclusion assumption. This condition requires that the instrument does not affect human capital and poverty outcomes directly, but only indirectly through its effect on X_i. In this setting, the exclusion restriction requires outcomes to be determined by CDH treatment, irrespective of the instruments realization.

Secondly, the instrument has to affect household's decision to change programmes. The 'first-stage' assumption asks the instrument to manipulate the probability of treatment, which means the probability of being a CDH beneficiary. Formally,

$$P(X_i|Z_i = 1) \neq P(X_i|Z_i = 0)$$

The left-hand side of this equation shows the participation rate of CDH programme for families with $Z_i = 1$, whereas the right-hand side shows the participation rate for

households with $Z_i = 0$. Since the participation rates differ, the instrument affects the probability of treatment (Angrist, 2004).

Monotonicity is the third assumption. Monotonicity requires all subjects that are affected by the instrument, to be affected in the same direction. Formally, either $X_{1i} > X_{0i} \forall i$ or vice versa. Without loss of generality, assume $X_{1i} > X_{0i} \forall i$, then subjects who would change to CDH if $Z_i = 0$ still change programmes if $Z_i = 1$ and some subjects who would not prefer the CDH if $Z_i = 0$ will change their decision and switch programmes if $Z_i = 1$. Notice that subjects are heterogeneous with respect to their response to the instrument and, therefore, households can be divided into three subsamples:

Always-takers are the ones who will switch programmes irrespective whether $Z_i = 0$ or $Z_i = 1$: $X_{1i} = X_{0i} = 1$. Never-takers neither change to the CDH if $Z_i = 1$ nor if $Z_i = 0$: $X_{1i} = X_{0i} = 0$. Finally, the group of compliers will prefer the CDH programme if $Z_i = 1$ and will not if $Z_i = 0$: $X_{1i} = 1$ and $X_{0i} = 0$. Thus, compliers treatment decision is determined by the instrument. To conclude, the monotonicity assumption, as specified here, does not allow for defiers. Defiers are subjects who decide to switch programmes if $Z_i = 0$ and will not if $Z_i = 1$: $X_{1i} = 0$ and $X_{0i} = 1$.

The resulting IV estimates can be interpreted as an average treatment effect from CDH to poverty and human capital for the Ecuadorian households that are beneficiaries of cash transfer programmes and comply with the instrument. These are the families who have decided to switch programmes, because they have previous credit experience that can be tapped. However, as explained above, there are also households in the sample who changed to the CDH programme regardless of the history of credit access (or level of experience) and others who would never switch programmes. Thus, although the results are internally valid, external validity and generalizability do not necessarily apply. In other words, the IV estimates cannot be easily applied to the entire study sample or population as heterogeneity in the treatment effect is possible. For this reason, the instrumental variable estimator is commonly referred to in the technical literature as a local average

treatment effect (LATE), because the average effect is only measured for the subpopulation of compliers and therefore applies only locally (Imbens & Angrist, 1994).[60]

The following equation shows that the Wald estimator on the left-hand side is equal to the LATE, the effect of treatment on those whose treatment status is manipulated by the instrument (Angrist, 2004):

$$\beta_{IV} = \frac{E(Y_i|\, Z_i = 1) - E(Y_i|\, Z_i = 0)}{E(X_i|\, Z_i = 1) - E(X_i|\, Z_i = 0)} = E(Y_{1i} - Y_{0i}|\, X_{1i} > X_{0i})$$

Note, however, that if there is heterogeneity in the CDH treatment effect, then this result is uninformative for the group of always-takers and never-takers (Angrist, 2004).[61] The LATE parameter measures the effect of the CDH treatment on individuals that move from the non-treated group to the treated group as the instrument *Z* changes. In this case, the LATE focus on a significant subpopulation of individuals and, therefore, it may provide an important measure of the effect of the CDH programme.

In a standard IV application, the researcher first selects a set of (assumed) exogenous variables (i.e. instruments) and then uses two-stage-least-squares (2SLS) or similar econometric techniques to estimate the coefficients in the regressions (Larcker & Rusticus, 2010). Once the instruments have been found and their validity has been well established, it is time to analyse the econometric procedure used to exploit this information. Representing the second part of my identification strategy, I will estimate different versions of the instrumental variable (IV) two-stage regression model described below.

Consider the following two equations:

[60] In particular, the instrumental variable (IV) approach neither identifies an average effect on the treated (ATET) nor does it identify an average effect for the whole population (ATE). See Harris and Remler (1998) for a relatively accessible explanation of the generalizability of instrumental variables estimates.
[61] Abadie, Angrist and Imbens (2002) extend this approach to the evaluation of *quantile treatment effects*. The goal is to assess how different parts of the outcome's distribution are affected by the policy.

$$Y_i = \beta_1 W_i + \beta_2 X_i + \varepsilon_{zi} \,, \qquad (2)$$

$$X_i = \theta_1 W_i + \theta_2 Z_i + \varepsilon_{xi} \,, \qquad (3)$$

where Y is any of the outcome variables under study; X is a binary variable representing the CDH treatment status (i.e. a dummy variable that equals 1 for poor households who took the decision to switch to the CDH programme); Z is the instrumental variable of previous credit experience; W is a vector of individual- and household-level characteristics; $\beta's$ and $\theta's$ are parameters to be estimated; and ε_{zi} and ε_{xi} are random errors that are uncorrelated with each other and with their respective independent variables.

One method of estimating the parameters of interest in Equations (2) and (3) is to use the fact that Z is an instrumental variable for X. First, I estimate (3) as a logistic or probit regression model (because the dependent variable is dichotomous) and, using the estimated equation, I calculate expected values for X:

$$E(X_i|W_i, Z_i) = \widehat{X}_i = \hat{\theta}_1 W_i + \hat{\theta}_2 Z_i$$

Then, in a second stage of the estimation, I replace X with $\widehat{X}$ in Equation (2) and estimate this equation by a method suited to the measurement of Y according to the type of dependent variable (for example, ordinary least squares if the dependent variable is continuous, ordered logistic if it is an ordinal discrete variable, probit if it is a binary, etc.) This method consistently estimates β_1 and β_2 under the assumption that ε_{zi} and ε_{xi} are uncorrelated with each other and with W and Z.

Finally, it is important to stress that the instrumental variables (IV) method is quite convenient in cases where you have strong arguments to believe in the validity of the instrument. As previously noted, this method works correctly only under the assumption that Z affects X but not Y; otherwise $\widehat{X}$ would be an exact linear combination of variables

already in (2) and β_2 would not be consistently estimable. In addition, for the method to yield precise estimates, *Z* and *X* should be strongly associated.

In research practice, however, suitable instruments are often difficult to find. This has led to a kind of "degradation" of the method using the so-called *weak instruments*, which are only moderately correlated with the regressors, and where the exogeneity assumption may be dubious. In these cases, it is possible to obtain a slightly more accurate estimate when samples are large (Stock & Watson, 2003). However, even then, the use of weak instruments does not definitely solve the problem, but rather redirects the discussion from the suspected endogeneity of the independent variable to the actual validity of the instrument. Therefore, IV estimation should always be used as a complement to, rather than a substitute for linear models; and if you still have reason to think that the instrumental variable conditions are not met, alternative quasi-experimental methods (such as DID estimation) should be considered.

3.3.2 Difference-in-Difference Method

A different quasi-experimental design that also seems feasible is to use a *Difference-in-Difference* strategy, exploiting the fact that the Registro Social process is repeated every 6 years and that I have access to two different databases (2008 and 2014). The data are not panel-type; therefore, respondents are not exactly the same in both datasets and this is an obstacle. However, the Registro Social is so large (around 9 million people) that certainly a lot of the respondents will be repeated in both databases. This is even more likely to happen considering that an important part of the target population is around the first and second quintile of the welfare index.

It is necessary to point out that it is possible (although somewhat difficult) to identify repeated respondents using the identity number reported in both databases. Since I have information at two different times, both for the treatment and control groups, it is possible to isolate the externalities and correct for potential self-selection bias and

endogeneity problems (associated with the CDH programme implementation) in order to calculate the real effect of the CDH treatment among BDH beneficiaries (Munoz, 2010).

The diff-in-diff strategy can be summarized in four crucial steps. First, it will be necessary to merge the databases (based on unique characteristics, such as the identity number complemented by age) to obtain a unique sample of individuals registered in both datasets. Second, I will construct sub-samples of people with similar characteristics (i.e., people living in poverty) that were never treated by the BDH or CDH programmes (control group). Similarly, I can construct two sub-samples of individuals treated by each one of the cash transfer programmes before 2014 (treatment groups). Note that in this step I will take into account the BDH and CDH programmes individually. Finally, I compare the BDH treatment group with the control group –and the CDH group with the BDH group– before and after the intervention in order to find the specific effect of a given programme on the living conditions of poor households.

This econometric approach is optimally used when there are two periods of data and a distinct treatment and control group (Munoz, 2010). This method aims to capture separately not only the differences between both groups, but also the changes over time (i.e. trends). Specifically, it compares changes over time in a group affected by the treatment intervention to the changes over time in a group unaffected by the treatment, and attributes the "difference-in-differences" to the effect of the intervention. As a result, it provides unbiased estimates of the effects if the trend over time would have been the same between the treatment and control groups in the absence of the intervention (Stuart et *al*., 2014).

Formally, we would have a treatment $(j = 1)$ and a comparison $(j = 0)$ group for both the before $(t = 0)$ and after $(t = 1)$ time period. Thus, the estimated regression model is the following:

$$Y_{it}^{j} = \beta_0 + \beta_1 W_t + \beta_2 T^j + \delta(W * T)_j^t + \beta_3 X_{it}^j + \varepsilon_{it}^j \qquad (4)$$

Where Y_{it}^{j} is the level of the outcome variable of a given individual i in group j at time t. T^{j} is a dummy variable that takes a value equal to 1, if the observed person belongs to the treatment group, and takes value equal to 0, if the person is in the control group. W_t is another dummy variable, which takes a value equal to 1 in the post-treatment period; and 0 otherwise. The diff-in-diff estimator is δ, the coefficient of the interaction between T^{j} and W_t. Note that this interaction term is also a dummy variable, which takes a value equal to 1, only for the treatment group in the post-treatment period. Lastly, X_{it}^{j} is a set of control variables and ε_{it}^{j} is the error term.

The other terms in Equation (4) are the coefficients $(\beta_0, \beta_1, \beta_2, \delta, \beta_3)$ of the diff-in-diff regression model. This regression is useful in deriving the treatment effects, namely, whether a given programme made a difference to Y_{ijt} in the treatment group after the intervention. The regression coefficients help in deriving different effects for the treatment and control groups. For example, if β_1 (i.e. the coefficient for the time dummy variable) is statistically significant, this means that part of the (presumed) differences between the treatment and control groups is explained by the passage of time; and therefore, in the absence of treatment both groups would have evolved over time anyway. Moreover, if β_2 (i.e. the coefficient for the group dummy variable) is statistically significant as well, this suggests that there are *intrinsic* differences between the treatment and control groups, which are therefore not explained solely by the treatment itself but by other causes (e.g. unobserved factors).

However, the most important coefficient is the one that captures the differences between before and after treatment, as well as the innate differences of the treatment group with the control group, that is the parameter δ (i.e. the coefficient for the interaction variable). If δ is statistically significant, after controlling for the independent effects of time and group, this means that there are differences between both groups caused exclusively by the treatment. In other words, the interaction coefficient shows the actual impact of the treatment. Mathematically, the estimate of δ would be the difference

between mean of Y after minus mean of Y before for the treatment group, and mean of Y after minus mean of Y before for the comparison group,

$$\hat{\delta} = (\bar{Y}_1^1 - \bar{Y}_0^1) - (\bar{Y}_1^0 - \bar{Y}_0^0).$$

Note that to estimate this coefficient, it is necessary to restrict the model so that,

$$E[Y_i^1|i,t] = E[Y_i^0|i,t] + \delta.$$

Therefore, the rationale for this double-differencing strategy can be also explained in terms of conditional mean functions for potential outcomes. It is important to note, however, that the main threat to this method is the possibility of interactions between group and time even in the absence of the intervention.[62]

In this methodological chapter, I have discussed the advantages and disadvantages of the natural- and quasi-experimental research designs proposed in this thesis. These quantitative methods are used to evaluate the impacts of the BDH and CDH cash transfer programmes on different outcome variables. Given that the identifying assumption of my natural experimental design is an "as if" random treatment assignment for both programmes, there is a latent concern for a potential self-selection bias. Therefore, it is necessary to address the possible endogeneity problems by using an IV approach and DID estimation. Now, it is time to focus on the data from the Registro Social, the necessary variables and the construction of a panel-type database that is essential for the econometric analysis.

[62] See Angrist and Kruger (1999) pp. 21.

CHAPTER 4
The Ecuadorian Registro Social Data: Constructing a Panel-Type Database

4.1 Introduction

This thesis focuses on the Crédito de Desarrollo Humano (CDH) and the Bono de Desarrollo Humano (BDH) programmes, which grew out of a much earlier programme known as the "Bono Solidario". The Bono Solidario was the first large-scale social assistance programme in Ecuador. However, as explained in Chapter 2, one of the most important problems faced by the Bono Solidario programme since its early stages was the lack of an adequate information system for the administrative process (Schady & Araujo, 2006). As a consequence, many of the beneficiary households were non-poor (i.e. leakage problems), and much of the poor population was not covered by the programme (i.e. poor focalization).

The clear need for better information and targeting mechanisms led to the further implementation of surveys with national coverage in order to achieve a more accurate identification of households in poverty conditions. The first survey wave was conducted between June and August 2003 (before the launch of the BDH programme) and a second

wave was conducted between January and March 2005 (two years after the BDH programme's application) (Schady & Araujo, 2006). However, it was not until August 2009 that the continuous application of surveys was institutionalized, through an Executive Order delegating to the Ministerio Coordinador de Desarrollo Social (MCDS) the responsibility for the establishment and management of a national cadastral survey named Registro Social (Social Register) (MCDS, 2016b).

The ultimate goal of the Registro Social (RS) was the formal establishment of a complete register –containing individualized social, economic and demographic information– which can be used as a recurring tool to determine the well-being situation of Ecuadorian families, so that they can have access to social programmes and different subsidies (MCDS, 2016). However, in order to create the necessary conditions for the integral protection of people throughout their life cycle, the state needs to have access to up to date data, with special attention to those individuals or groups who have been traditionally excluded (i.e. the poor, disabled, and elderly populations).

For this reason, every six years a follow-up cadastral survey is carried out, covering more households in each round with the purpose of assessing whether families are still in a situation of poverty, as well as identifying potential beneficiaries of the different welfare programmes offered by the Ecuadorian Government. As a result, meaningful data is available for public sector management as a basis for decision-making. The academic use of these data is recent and limited because so far no RS database has been published for public review. Moreover, according to government officials, until recently the data collected through the RS surveys have been used exclusively by the State's social sector and some academic institutions (MCDS, 2016b).

The quantitative data used in this thesis are constructed from the large-scale household surveys conducted as part of the last two RS rounds of data collection. These official records are distinctive in the sense that they are based on the direct registration of households' conditions with the explicit purpose of knowing better the target population in order to focus resources to them, including factual information on access to welfare

programmes as well as individuals' perception on their own poverty situation. More specifically, the datasets contain comprehensive information on social benefit recipient and applicant households, which allows easy recognition of those eligible to participate in the BDH and CDH cash transfer programmes (Palacio, 2014).

Therefore, I use official records of the RS as the sampling frame in the process of implementing my research. Following Mortelmans and Pasteels (2013), there are significant gains of using official records (i.e. secondary data sources). The authors mention the following specific advantages: data on the target population is frequently accessible, comprehensive and structured, and what is more, almost without missing values.[63] Thus, in general, the official nature of the data facilitates social research both by saving valuable time –given that the necessary information has been already collected by public agencies– and by supporting any further analysis on formal institutional grounds, since the data has been gathered by the bureaucratic apparatus of the state.

Nevertheless, relying on secondary administrative data has its own disadvantages and this case is no exception. Palacio (2014) identifies some of the problems of relying specifically on the RS official records for conducting social research. As the author fielded the survey and looked for some of the households selected from the sample, she noticed that the data is not fully accurate, important contact information is missing, addresses are sometimes obsolete and household composition data is in some cases outdated. For example, according to the author, certain groups –such as informal workers– were extremely difficult to find and remain misrepresented in the official records. Also, she points out that many new-borns are not registered, leading to an underestimation of the size of the household.

But perhaps the most important shortcoming identified by Palacio (2014) is the potential presence of misreporting problems –mainly due to people's general knowledge about the final purpose of the surveys. The author notices that, for example, some people

[63] A full review on the missing values in the RS databases and the possible strategies for dealing with them is discussed in a later section.

were fully aware that the lack of formal employment is actually a good strategy to access social assistance benefits. Besides, a significant number of surveyed individuals know that cash transfer programmes are targeted exclusively to poor households, and therefore, they are quite reluctant to talk about income sources and other benefits at work (such as social security) so as not to reduce the chances of being selected.

All considered, the RS remains the most complete and the most updated source of secondary information available in Ecuador. Note that the potential shortcomings described above are also common to most survey-based social research. As such, it is important to keep in mind the possibilities of misreporting and measurement error when interpreting the findings in order to avoid drawing incorrect conclusions.

4.2 The Instruments of the Registro Social

At this point, it is important to recognize that the term "Registro Social" can be understood simultaneously as a simple survey, a comprehensive database, or even as a complex system used to rank households within the country. Conceptually, the RS could be described as a national survey used to distinguish the socio-economic status of households and individuals –focusing particularly on the most vulnerable areas– with the purpose of guiding the coordinated actions of the social sector in order to respond efficiently to the constant needs of the population. Formally, the MCDS (2016) defines the RS as "a socio-economic cadastre of Ecuadorian households and their members, which allows social assistance programmes to identify their target populations (i.e. a targeting mechanism), and thus articulate the actions necessary to achieve the upward social mobility of these households over time".

As a system, the RS is based on two main instruments: a record of socio-economic information from households known as RCS (Registro de Caracterización Socio-económica); and a welfare index known as SELBEN (Sistema de Identificación y Selección de Beneficiarios de Programas Sociales) (MCDS, 2016b). The RCS record is

used to compile a database with individual and household level information (i.e. Registro Social administrative database). This database is used in turn to create the SELBEN welfare index, which ranks Ecuadorian households according to social, economic and demographic characteristics. As will be explained in more detail in a later section of this chapter, the welfare score of a given household –according to the SELBEN– is computed using a nonlinear principal components analysis based on 59 different variables, including housing characteristics, household assets (television, car, telephone, etc.) and characteristics of the members of the household (schooling, ethnicity, illiteracy, employment status, etc.) (Fabara, 2009).

The RS database is used to understand the well-being levels of Ecuadorian families on important issues such as health-care, education, housing, among others. This administrative database contains important human capital-related information, such as school enrolment, school assistance, educational attainment and child care (i.e. frequency of attendance at clinics for children's medical check-ups). Additionally, it comprises some common poverty indicators related to living conditions, overcrowding, and access to basic services. There are also data on demographic and socioeconomic characteristics of households and individuals, such as region, province, city, district, identity number, age, marital status, language, economic activity, and labour supply (MCDS, 2013).

However, perhaps the most important feature of the RS database –at least for the purpose of this paper– is that it includes information about engagement in social assistance programmes, such as having been a BDH or CDH beneficiary, and even the reported use of cash transfers (investment in health, education, household equipment, debt repayment, nutrition, business entrepreneurship, or personal expenses) (MCDS, 2013). Therefore, given the vast amount of available data, a comprehensive review of the survey questionnaire was necessary –as an important preparatory step– to determine the most appropriate outcome and explanatory variables for comparing the effectiveness of the two cash transfer programmes considered in this study. A complete list of the variables selected for the quantitative evaluation is included in the last section of this chapter.

The Registro Social's instruments described above (namely, the administrative database and the SELBEN welfare index) are used together by government agencies to identify potential beneficiaries of the cash transfer programmes (MCDS, 2016b). More specifically, each social assistance programme independently sets the value of the index below which the registered households can receive state subsidies or social benefits. For example, until 2014, the BDH programme delivered cash transfers "as if" randomly but only to households with a score of 36.5 or lower according to the SELBEN (i.e. those in the first two quintiles of the poverty index). The individual evaluation aims to reduce the leakage of ineligible beneficiaries (for instance, those who are not in poverty).

4.3 Methodology and Coverage of the Registro Social

The Registro Social is, by definition, a cross-sectional survey that is used to gather information on a target population at a single point in time. The methodology used for data collection through field operations is of three types: by sweeping, by group call and by demand (MCDS, 2016b). The first method, known as "sweeping", is the one used in most cases and consists in collecting information by visiting directly the households in their respective homes, which are located in each of the intervention sectors previously identified by the RS managers.

On the other hand, the "group call" method is used when census districts are widely dispersed (as in the Amazon region) and it consists in gathering information by appealing to the resident populations of some selected sectors, and thus, concentrating the people in a certain place to be interviewed individually. One of the disadvantages of the surveys by group-call is that it is not possible to verify some information (e.g. the housing characteristics) by direct observation. However, this is not a major issue since the vast majority of records are obtained through the sweeping method. In fact, according to Martínez, Borja, Medellín and Cueva (2017), during the last RS process, less than 10 percent of household information was collected through the method of group-call.

Alternatively, there is also the collection of data on-demand, which is requested by families who have not been properly registered during previous field operations. This may be due to circumstances such as: household members were not at home when they were visited; respondents were not qualified to provide information; interviewers did not reach a household due to difficulties in finding the residence where they live. Also, the on-demand registration procedure allows households residing in census districts that are not identified as poor to apply for participation in the BDH. In order to be surveyed, families must voluntarily request a home visit contacting the MIES. This could be done by telephone –contacting the institutional call centre–, directly at MIES offices or by entering to the RS official web page and registering the household's full address along with the individual information on the household members (MCDS, 2016). In these cases, the MCDS normally waits for several cases to accumulate in each census district to carry out an information gathering field operation.

Remarkably, in the last RS process, electronic tablets were used for the first time to apply the surveys (Martinez et *al*., 2017). This important innovation allowed the MCDS to improve the level of planning of the field operations and their outcomes. For example, it was helpful for designing better routes and increasing the quality of the information. The use of tablets also permitted the collected information to be simultaneously compared with different administrative databases. Thus, for instance, it was possible to validate if the surveyed person had an updated identity card number. In the case of the families surveyed by the sweeping method, the location of the houses was geo-referenced and even a photograph of the house was added to each family's file, which makes it easier to follow up the surveyed households on future RS processes. In addition, through an application software specifically designed by the MCDS, the probability of common mistakes while implementing the surveys –such as filling errors and the omission of information by the interviewer– was reduced. Finally, at the end of the interview, the employed software generates an electronic document that was signed by the interviewee to confirm the veracity of the information.

The field operations of the Registro Social 2008 (namely, sweeping and group-call data collection methods) were carried out from December 2007 to June 2009 (MCDS, 2016b). According to the MCDS official records, about 80 percent of the information was collected by the Instituto Nacional de Estadísticas y Censos (INEC) and the remaining 20 percent by the MCDS. Moreover, about 70 percent of the first RS data was obtained through the sweeping methodology in the selected areas and 30 percent was collected by group call. Finally, between November and December 2009, the RS survey was carried out on demand. At the end of the on-demand field operations, it was estimated that approximately 180,000 new homes were incorporated into the database.

The data collection process during the last RS in 2013-14 was conducted in a very similar way (i.e., emphasizing the first two types of field operations). As a result, 72 percent of the data was gathered using the sweeping method in selected areas throughout the country. This represents nearly 1.44 million Ecuadorian households that were surveyed by the INEC and the MCDS from September 2013 to July 2014. Most of the remaining 28 percent of the data (representing 0.56 million households) was collected by group calls in those sectors that are more difficult to cover through direct visits to households (MCDS, 2016).

For the application of the Registro Social survey process, the MCDS uses the recently established administrative division of the country into census districts. According to this division, the Ecuadorian territory is currently organized into 42,649 census districts, each of which contains approximately 100 to 150 households (MCDS, 2016b). Prior to the implementation of the first RS survey, it was estimated that a total of 3,392,851 households (i.e. 14,321,699 individuals) were spread across the census districts at the national level. Moreover, it was also estimated that 38.3 percent of the total population were living in poverty or extreme poverty at that time (i.e. 1,319,819 poor households) (MCDS, 2016b).[64] According to the INEC (2010), poverty in the country is almost equally

[64] The INEC traditionally determines the poverty status of each household by using the Unsatisfied Basic Needs Methodology (UBN). According to this approach, living conditions are measured with the help of the following indicators: 1) overcrowding: more than three people living in one room; 2) water: homes

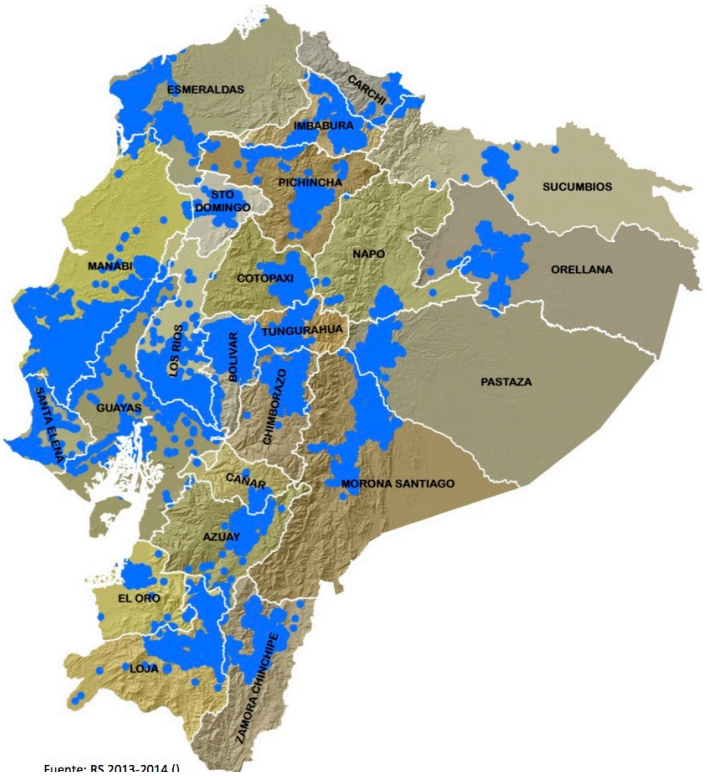
distributed between the urban (48 percent of households are in poverty) and the rural areas (52 percent). However, it should be noted that rural areas traditionally include suburban sectors.

Therefore, in order to efficiently carry out the first RS, the MCDS identified and selected 25,942 poor census districts (i.e. about 80 percent of the 32,129 districts that were established at that time). While in the last survey of socioeconomic information that was implemented in 2013 prior to the last RS process, 24,482 poor census sectors were identified out of a total of 42,649. These poor districts were chosen by using the Unsatisfied Basic Needs (UBN) methodology, according to which a census district is considered poor –and therefore taken into account– when it presents a UBN score equal or higher than 50 percent (MCDS, 2016b).

The MCDS (2016) reported that most of the households in the poor districts are visited during each RS survey wave. Typically, a high percentage of them are effectively surveyed at the end of the process, thereby obtaining the socio-economic characterization of nearly two million Ecuadorian families. For instance, between September 2013 and July 2014, the RS reached an outstanding daily production of 8,500 records. As a result, a total of 2.1 million households were visited and more than 2 million registers were fully completed along the national territory (MCDS, 2016). Figure 6 depicts the geographic localization of the poor census districts that are traditionally surveyed every six years by the RS.

without access to water public system; 3) toilet facility: households without sewage; 4) schooling: households with children of school age (6-12years) without access to primary education; and 5) health: no household member has access to public health insurance. Note that this method allows defining the threshold for each indicator considering the surrounding environment, culture, and social conditions of each specific population (Feres & Mancero, 2001).

Figure 6: Geographic Localization of the Surveyed Poor Districts



Source: MCDS (2016).

As can be seen in Figure 6, the RS survey process covers a significant portion of the national territory. However, it is important to stress that the surveyed samples are *not* representative of the *whole* national population because, as explained above, the process is carried out mainly in the areas (or census districts) with the highest poverty rates within the country (i.e. focalized surveys). In other words, the surveys' target population – represented by the RS samples– are only those individuals considered as potential beneficiaries of social assistance programmes and, therefore, the process is conducted only in zones with a high incidence of poverty (MCDS, 2016b).

Remarkably, the last RS covered 2,130,765 households (i.e. 9,485,132 individuals), each of which is consistently assigned a specific score in accordance with the methodology established for the SELBEN welfare index calculation (which is

explained in the section below). In this thesis, I use household- and individual-level data taken directly from the Registro Social's 2008 and 2014 databases. However, as previously explained, an important technical issue –that must be considered for the quantitative part of the analysis– is that the RS is not a panel-type database.

Fortunately, according to the Registro Social's official administrators, the level of attrition between the last two cadastral surveys is relatively low. More specifically, it is argued that about 70 percent of the households registered in 2008 were interviewed again six years later in the survey wave conducted between 2013 and 2014 (MCDS, 2016). Besides, it is possible to identify the repeated respondents by using the identity number, which is conveniently coded and reported in both databases. These assertions are (for the most part) confirmed in a later section of this chapter when an artificial panel-type database is successfully created using the two available RS datasets.

4.4 The SELBEN Welfare Index

The SELBEN index (or Welfare Index) is frequently employed as an indicator or proxy for consumption poverty (MCDS, 2016). Since its implementation in 2008, many different variables have been used for the construction of the index, which are all obtained through the RS database. These variables are (to different extents) correlated with the level of household consumption and well-being. Moreover, the methodology used for the estimation of the SELBEN index is that of non-linear principal components (Fabara, 2009). This methodology allows families (and their members) to be classified socioeconomically using a model based on a set of consumption-related variables.

More specifically, the model used by the MCDS to construct the Welfare Index is based on six main components of households' consumption levels, each of which has a different percentage of participation and includes several variables (MCDS, 2016). The specific weighting of each component is set in such a way that the poverty index presents values between 0 (for those households in worst poverty conditions) and 100 (for those in

the best conditions). In 2014, through the obtained values by the index, it was estimated that 11.5 percent of the households at the national level were living in extreme poverty (i.e. households with an index value less than or equal to 24.07 points) (MCDS, 2016).

For the selection of the multivariate method that allows the formulation of the Welfare Index, the MCDS started analysing the results obtained with several multidimensional methods, including: regression analysis and discriminant analysis (Fabara, 2009). In fact, different theoretical models can be successfully applied to statistically classify households. For instance, the application of income-related models was initially considered by the MCDS statisticians. However, due to the great variability and temporality of the obtained results, this kind of models were discarded (Fabara, 2009). It should be noted that the main objective was to develop an index that is both stable over time and based on structural variables. Therefore, once the available information and the results obtained with different models were taken into account, the MCDS opted for the non-linear principal components analysis (PRINCALS), which has been widely used for its great power of discrimination in this type of classifications (MCDS, 2016).

Initially, the MCDS used the Registro Social database to select a set of 59 variables to be part of the chosen PRINCALS analysis. Fabara (2009) points out in her institutional report for the MCDS –*Reformulation of the Social Register Index of Socioeconomic Classification*– that for the adequate design of the poverty index and the correct selection of the variables, the MCDS considered three elements: the academic literature on this subject, the level of linear association of each variable with per capita consumption at the national level (calculated using the Pearson's correlation test), and the social assistance's beneficiary selection models applied in other countries (which are also based on the socioeconomic classification of households).[65]

[65] The Pearson correlation coefficient, whose value does not depend on the units of measure of the variables, is bounded by -1 and +1. The sign of the coefficient indicates the direction (positive or negative) of the linear association and the absolute value the intensity of it.

As a result of this analysis, the variables used in the model basically encompass housing characteristics complemented with some indicators related to the household's composition, the characteristics of the head of household and the availability of goods. These variables have the peculiarity of being stable over time and they are mostly structural. In addition, the issue of territoriality is included in the model, thus it considers the geographical location of the household and the level of consumption poverty in each census district. The table below details the 59 variables considered by the non-linear principal components model for the elaboration of the Welfare Index in 2008. Here, these individual- and household-level variables are classified according to their nature or typology (e.g. discrete or continuous).

Table 7: Variables of the Registro Social Employed for the Construction of the SELBEN Welfare Index 2008.

Type	Variable	
Categorical (3 or more categories)	1. Geographical location	2. Reason why children are not enrolled in school
	3. Ethnic self-definition of the head of the household	4. Educational attainment of the head of the household.
	5. Number of cell phones in the household	6. Housing tenure
	7. Number of migrated household members	8. Access to electricity
	9. Occupational category of head of household	10. Ownership of land for agriculture
	11. Type of fuel used for cooking	12. Type of educational institution attended
	13. Use of cash loans	14. Type of toilet facility
	15. Availability of shower facility	16. Type of housing
	17. Marital status	18. Ownership (title) of the property
	19. Housing general condition	20. Type of water treatment system

	21. Method of garbage disposal	22. Location of water and sanitary services
	23. Type of water source	24. Main access road to housing site
	25. Language spoken by the head of household	26. Main type of floor material
	27. Main type of walls material	28. Main type of roof material
Dichotomous (2 categories)	29. Economic activity of the head of household	30. Household receives pension income
	31. Access to insurance	32. Holding of farm animals
	33. Overcrowding status	34. Access to gas water heater
	35. Household receives rental income	36. Access to internet service
	37. Household receives scholarship income	38. Access to cable TV
Continuous	39. Years of schooling of head of household	40. Number of colour televisions in the household
	41. Number of cars in the household	42. Number of VHS/DVDs in the household
	43. Number of cooking appliances in the household	44. Age of the Head of the Household
	45. Number of computers in the household	46. Household receives income from family support services and NGOs
	47. Number of washing machines in the household	48. Number of working children
	49. Number of food blenders in the household	50. Number of people in the household
	51. Number of telephone land-lines	52. Number of people between the ages of 18 and 64 earning income
	53. Number of children under 6 years old in the household	54. Average consumption poverty of the census sector
	55. Number of microwaves in the household	56. Access to cash remittances from abroad
	57. Number of irons in the household	58. Number of refrigerators in the household

	59. Amount of loans received in the last twelve months

Source: Own elaboration based on Fabara (2009).

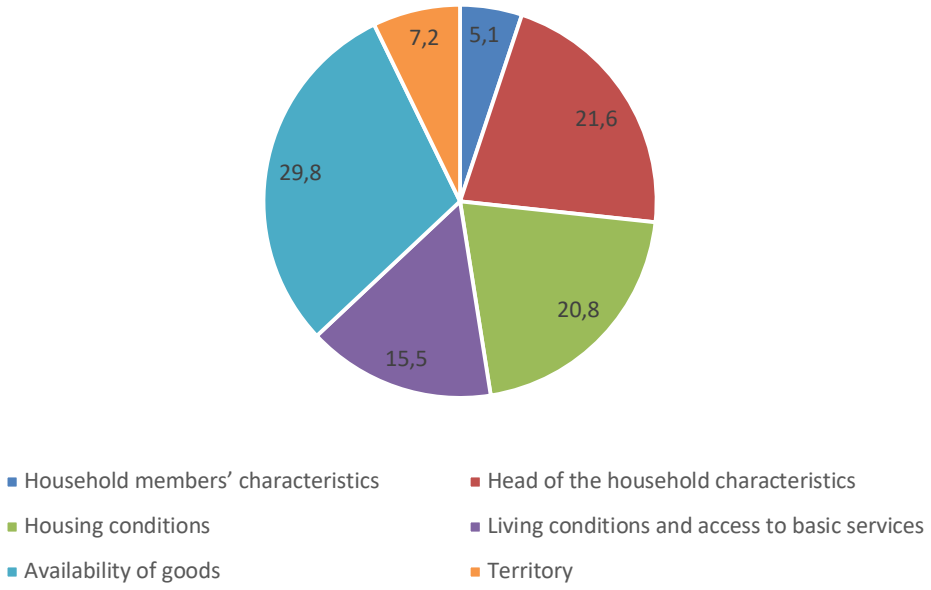
As can be observed, these variables are directly correlated with the household's level of consumption and well-being. For the accurate estimation of the welfare index, the 59 variables are grouped into six different components or categories. The number of variables included in each component is presented in Table 8. Additionally, Figure 7 shows the percentages of participation (i.e. weighting rates) of each of these components in the Welfare Index. The percentages are institutionally determined by the MCDS as part of the PRINCALS methodology.

Table 8: Number of Variables by Non-linear Component (2008 Methodology)

Component	Number of Variables
Household members' characteristics	6
Head of the household characteristics	9
Housing conditions	15
Living conditions and access to basic services	15
Availability of goods	12
Territory	2
TOTAL	**59**

Source: Own elaboration based on Fabara (2009).

Figure 7: Proportion of Each Component in the Welfare Index 2008

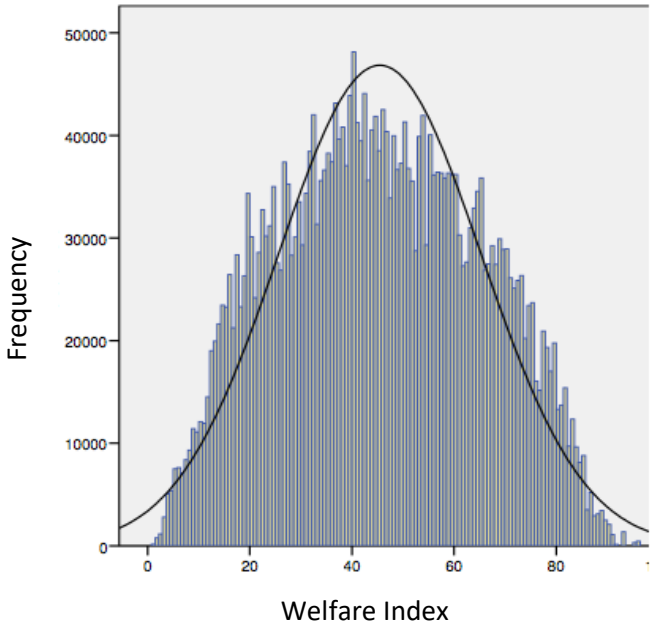


Source: Own elaboration based on MCDS (2016).

It should be noted that, in 2014, the MCDS changed the consumption-related variables and, thus, some of the components used for the construction of the Welfare Index. Specifically, for the non-linear principal components analysis, the number of variables was reduced to 34 and the participation percentages were also modified (MCDS, 2016). Although the poverty index introduced in 2014 presents methodological advantages and may be more precise, this thesis uses only indexes constructed with the initial methodology for reasons of comparability. Note that while it is possible to apply the initial methodology in the 2014 database, the opposite is not feasible because the 2008 survey includes a smaller number of questions (i.e. database variables). Fortunately, the latest Registro Social includes welfare indexes calculated with both methodologies (i.e. 2008 and 2014). Nevertheless, as will be seen below, it was necessary to standardize (or make equivalent) the selected variables of the Registro Social 2008 and 2014, because in some cases the response categories were not equal in both databases.

Finally, according to the general statistical theory of normally distributed samples, the distribution of the Welfare Index should be approximately normal given the large number of observations. In order to verify this assumption, I present below the distribution of the survey sample –considered for the application of the Registro Social 2008– according to the Welfare Index (see Figure 8). By plotting the histogram, it is possible to get quite a clear picture of the distribution of the index-estimator. As can be seen, the sample is normally distributed and the dispersion of data is significant throughout the index scale.

Figure 8: Histogram of the Distribution of the Welfare Index in 2008



Source: Fabara (2009).

4.5 Creating a Panel-Type Database from Two Different Registro Social Datasets

For the analysis to take place, I gained access to the two different RS databases available (i.e. 2008 and 2014). The Ecuadorian government, through the MCDS, approved and provided the necessary information to carry out this research project. However, the process of acquiring the data was not easy and (as expected) a series of difficulties arose during this stage, which was one of the first in the development of this thesis. First of all, the bureaucratic difficulties that typically arise when it comes to gaining access to data, especially in less developed countries with information systems that are not well integrated. Given that the information in the Registro Social is confidential and not open to the general public, it was necessary to begin by writing a formal letter to the Minister of Social Development in July 2014. This letter specifically explained the type of data needed and the purpose of the investigation. It was delivered through the Ministry of Education –which also financed my doctoral studies– with the aim of accelerating the process and minimizing the possibility of future complications.

Once the request was approved by the end of 2014, I went to Mr. Reinaldo Cervantez, manager of the Registro Social, with whom I personally met on different occasions and exchanged some emails until we reached an agreement on important details, such as the quantity and quality of information that will be provided and the time it would take them to deliver the databases. Mr. Cervantez explained that the approximate waiting time would be between 4 and 6 months, mainly due to the heavy workload that the MCDS had at that time and because it was necessary to carry out some previous work with the databases. More specifically, they had to codify in some way the identification number of the individuals, as well as to standardize the SELBEN Welfare Index so that it would be possible to compare it between two different years. In addition, he mentioned that the waiting time would be longer depending on the size of the databases requested, that is, the number of observations and variables.

Therefore, before making important structural decisions about the requested databases, it was necessary to perform a comprehensive review of the almost 300 variables that make up the full Registro Social databases. Note that when combining data from two timepoints, the number of variables is doubled. For example, we have the individual-level variable 'educational attainment' in both 2008 and 2014. Appendix C summarizes and categorizes some of the most important variables of the RS database, taking as a reference the survey questions in 2014. Of course, this summary includes all the independent and dependent variables that will be used in the regression analyses of this thesis (presented in later chapters) to estimate the effects of the BDH and CDH programmes.

Due mainly to time constraints, a much smaller number of variables were eventually requested to the MCDS as part of the RS databases (those considered essential for the impact evaluations). In order to select (a priori) the best possible variables for the analysis, the main information needs were taken into account, as well as the specific regression models that were planned to be estimated. The selected variables and the type of data effectively available are described extensively in Section 4.7 and, subsequently, they are used as independent and dependent variables in the different regression analyses. However, as will be described in more detail below, some of these variables had to be reshaped or simply standardized for comparison reasons (see page 174). For the quantitative analysis, for example, it was necessary to derive some useful variables that were not included in the original datasets (such as a households' unique identifier and the average educational attainment of the household). Moreover, it was important to standardize the variables of the Registro Social 2008 and 2014, because the response categories included in some variables were not equal in both databases. Therefore, I agreed with Mr. Cervantez that I was going to actively collaborate with the MCDS by writing the STATA codes needed to accelerate the process of preparing the databases and the selected variables. This was done through a joint work for a couple of months between an MCDS technician (Mr. Fernando Galvez) and me.

After about 6 months (mid-2015), I finally had access to the two complete RS databases with the variables requested. At this point, some problems of a more technical

nature were presented (such as the construction of a panel-type database and the necessary variables for data analysis) that, fortunately, were resolved without major inconveniences. Note that the RS data is not strictly panel-type; therefore, respondents are not the same in both registers -which constitutes a clear first technical obstacle for the evaluation. Fortunately, each RS is so large (around 9 million people) relative to the whole population that certainly a considerably number of respondents will be surveyed in both years. This is even more likely to happen considering that most respondents live in areas considered "threatened" or poor and, consequently, it is expected that they present scores around the first and second quintile of the welfare index. It is possible to identify repeated respondents using the unique identity number coded and reported by all surveyed individuals in both databases.

Once I managed to merge the two datasets in the program STATA, using the individuals' unique identification code as the common variable between registers, I obtained a complete panel-type database of repeated individuals composed of 3,394,210 observations for each measured variable. This total number of individuals corresponds in turn to 1,722,473 repeated households. In order to better understand the composition of the resulting panel data (for 2008 and 2014), individuals are classified according to different categories based on personal characteristics, such as age, race, gender, reported disabilities and poverty situation (i.e. welfare index score). The following table summarizes the distribution of individuals by age group (distinguishing between children and the elderly), by reported disability status, by poverty situation and by social assistance programme enrolment.

Table 9: Distribution of Individuals by Category (Percentage in Parentheses)

By Age Group	
Working-age adults (18 to 65 years old) in both timepoints	2,474,203 (72.89%)
Children aged <18 in 2008	530,249 (15.62%)
Elderly aged >65 in 2014	387,949 (11.42%)
Missing in both timepoints	1,809 (0.05%)
Total	3,394,210 (100%)
By Disability Status	
Reported disabilities in 2008 or 2014	256,735 (7.56%)
Without disabilities in both timepoints	3,135,666 (92.38%)
Missing in both timepoints	1,809 (0.05%)
Total	3,394,210 (100%)
By Poverty Situation in 2008 (i.e. baseline year)	
First two quintiles of the poverty index	2,347,880 (69.17%)
Above the poverty threshold	1,044,521 (30.77%)
Missing	1,809 (0.05%)
Total	3,394,210 (100%)
By Social Assistance Programme Enrolment in 2014	
BDH beneficiaries	740,727 (21.82%)

CDH beneficiaries	382,074 (11.25%)
No programme	2,012,396 (59.28%)
Missing [66]	259,013 (7.63%)
Total	3,394,210 (100%)

As this table indicates, 72.8 percent (2,474,203 individuals) of the two-times surveyed individuals are in adulthood in both timepoints and, therefore, they are part of the country's economically active population (EAP) for the entire period from 2008 to 2014.[67] For the purposes of this research, the economically active population is defined as those individuals (individuals between 18 and 65 years of age) who are working or are looking for work. This amount represents a considerable portion of the country's total population taking into account that, according to the National Institute of Statistics and Censuses, the whole economically active population in Ecuador is approximately 8 million people in 2016 (INEC, 2016). On the other hand, children, young adults and the elderly account for approximately 27 percent of individuals in the panel database.

The distribution of individuals by disability status shows that there are over 250,000 people (i.e. 7.5 percent) who reported any type of impairment or disability in at least one of the survey waves. The most commonly reported impairments are those that affect mobility and, although not included in Table 9, it can be pointed out that the prevalence of disability rises with age. Around 5 percent of children are disabled,

[66] In the case of variables indicating enrolment status in social assistance programmes, the number of missing values is somewhat higher (namely, 259,013 missing observations). However, almost all individuals with missed observations present well-being levels above the first two quintiles in 2008 and, as will be seen during the construction of the evaluation samples, these people are not relevant for the analysis since they are not potential beneficiaries of the programmes.

[67] Those who transition between timepoints are not considered economically active in the entire period for the purposes of this thesis.

compared to 9 percent of working age adults. Moreover, according to this database, about 3 million people -representing 92 percent of the surveyed population- reported no disabilities during the years 2008 and/or 2014. Therefore, the ratio of people without disabilities to those with disabilities is significantly large and, as will be seen in a later section, this fact is useful for the analysis of the data since it will not be necessary to eliminate many individuals during the construction of the sample for the evaluation.

Individuals in the database are also classified according to their poverty situation into two broad categories: above or below the poverty line (set at 36.59 points of the SELBEN welfare index). In 2008, people with a score within the first two quintiles of the poverty index make up to about 70 percent of the surveyed population (2,347,880 individuals). Meanwhile, individuals above the poverty line account for the remaining 30 percent (about 1 million people). This percentage distribution seems very logical considering that the target population of the RS is mainly composed of those people living in marginal areas of the country, i.e. areas considered with a high incidence of poverty.

This distribution is quite convenient for the subsequent quantitative analysis given that only those individuals in the first two quintiles are comparable (namely, potential beneficiaries of the cash transfer programmes), which represent about two thirds of the individuals in the panel database. Note also that Table 9 describes the distribution of individuals in 2008 because this year will be the baseline used for the evaluation. In other words, the distribution in 2014 may or may not change as a direct consequence of the application of social assistance programmes (i.e. BDH and CDH).

At this point, it is also useful to describe the distribution of individuals according to their enrolment status in the cash transfer programmes offered by the government. The last section of Table 9 shows that almost a third of the people in the panel database reported having been beneficiaries of the BDH programme (i.e. about 1.1 million individuals). Of these, 382,074 people decided to join the CDH initiative sometime between 2008 and 2014. Therefore, about 2 million people reported never having benefited from social assistance programmes during the six-year period. This amount is

quite considerable taking into account that only 1 million people were above the poverty line in 2008. In other words, out of the 2.3 million potential programme beneficiaries initially identified (i.e. in the baseline year), only half of them reported in 2014 having received cash transfers at some point.

The seeming low coverage can be explained by the strong budgetary constraints faced by the administration of these programmes. These budget constraints often prevent full coverage of the target population, as has historically been the case of the BDH and CDH initiatives. However, there is also the possibility that the problem of low coverage, reflected in the last distribution, is due to *misreporting* issues among surveyed individuals. It is important to recognize that sometimes people prefer not to report the benefits they receive from the government due to the justified fear of losing them in the near future if their answers are completely honest.

In general, the usefulness of any survey or census data for evaluating social programme outcomes could be affected by misreporting of participation (Kirlin et *al*., 2013). This is often solved by researchers using experimental research methods, which address misreporting problems by allowing the allocation of benefits in a controlled manner (Shady & Araujo, 2006). In the case that a controlled experiment is not an option, misreporting issues could be also addressed by combining administrative records with survey data. According to Kirlin et *al*. (2013), the potential advantages of such linkage are extensively acknowledged in social policy research. Nevertheless, this alternative to solve possible misreporting issues goes well beyond the practical possibilities of this thesis in terms of the available data and financial resources. I mention it simply as a future possibility to strengthen the validity of the obtained results.[68]

[68] Fortunately, the Registro Social process already gives special attention to verifying the accuracy of the information provided by the households. The information is cross-referenced with data from the social security administration, the public electricity company and the national credit bureau (De la O, 2015).

4.6 Missing Observations and Data Quality

In principle, the amount of missing data does not seem to be an important problem for the quantitative part of the evaluation. In fact, for most of the variables considered in the analysis only 0.05 percent of the observations are missing (see Table 9). These missing data rates are a measure of the level of unit response and they are used as an indirect indicator of the quality of the data (Bouza-Herrera, 2013). Moreover, the missing values of these variables come mostly from the same 1,809 individuals, which makes it difficult to check whether the missing values follow a particular pattern or whether they respond to certain individual characteristics. However, in order to effectively deal with any potential problem due to non-responses, it is necessary to better understand the possible reasons why some of the information is lost and, as far as possible, the actual distribution of missing data. Only in this way, it is feasible to decide correctly on the best analysis strategy to yield the least biased estimates.

Let's start by pointing out that missing observations and data quality improvement are well-recognized issues in statistical inference and quantitative data analysis (Hansen & Hurwitz, 1946). Missing data problems are present in social science surveying specifically because some interviewees are unwilling or unable to answer all the questions raised in the surveys (Särndal & Lundstrom, 2005). The reasons why some individuals are not available (or refuse giving information) may be several. However, they all of them have a common explanation, which has to do with the inherent complexity of survey research. Sampling and census survey practices involve a series of considerations that must be taken into account even before a survey can be developed.

Definitions, concepts and methods (like those for collecting and processing data) must be determined beforehand and have important repercussions. All these factors together create a working system, which is shaped by the specific aims of the survey. As a result of such a complex process of preparation and application, it is common that survey data are not collected for all the units in the sample or population under study. In fact, full response surveys are rare –almost utopic– situations (Bouza-Herrera, 2013). The usual

set-up is to find that there are missing data for a part of the population and problems appear at the moment when conclusions are drawn on that population using statistical methods.

Missing data is an umbrella concept under which are grouped all cases where the value of the variable of interest is not obtained. Following Bouza-Herrera (2013), in this thesis –like in any other survey-based research– missing observations are present when one or more of the following situations occur:

1. An individual of the target population is not included on the survey's sampling frame (non-coverage);
2. A unit in the sample does not participate in the survey (total non-response);
3. A sample unit fails to provide acceptable responses in some of the outcome variables (unit or item non-response).

As can be noticed from Table 9, most of the identified missing observations in the constructed Registro Social panel database fall into the category of total non-response, which is important to consider in order to decide next the best strategy for handling the missing data. There are some cases of item or unit non-response, which correspond almost exclusively to the explanatory variable of BDH status (i.e. 259,013 missing observations). However, the vast majority of these missing sampled elements (individuals) are well above the poverty threshold, which prevents them from being potential beneficiaries of any social assistance programme (i.e. BDH or CDH cash transfers). Therefore, it will not be necessary to take them into account when trying to compensate for missing data, given that they will not be part of the final samples used for data analysis.[69]

There are different possible ways to handle missing data, in order to ameliorate its impact on the accuracy of survey-based inferences (Bouza-Herrera, 2013). Actually, a number of statistical methods have been developed with the intention of compensating for

[69] Recall that, for comparison purposes, it is desirable to have two initially homogeneous groups (i.e. one treatment and one control) in terms of the outcome variables.

missing data.[70] Case deletion (Enders, 2010), weighting adjustments (Heyting et *al*., 1992), imputation of missing values (Peng & Zhu, 2008; Nakai & Ke, 2011) and subsampling the non-respondents (Hansen & Hurwitz, 1946), are four of the most popular approaches for dealing with the problem of non-responses. Each of these approaches is in turn subdivided into different methods, each with its own advantages and disadvantages.

In this thesis, given the characteristics of the Registro Social's data, the small amount of missing values (1,809 observations per variable) and the type of non-response (i.e., total non-response), the chosen method for handling missing data is complete case deletion (CCD).[71] The described characteristics of the missing data in the Registro Social panel database indicate that the quality of the information does not represent a serious problem for the analysis. Therefore, the simplicity and comparability (across different kinds of statistical analysis) of this method has been preferred over other more complex methods that in this specific case would not provide significant benefits in terms of statistical inference testing.

Complete case deletion, also known as list-wise deletion, analyses only cases (individuals) with available data on all the variables included in each model specification, while those with incomplete or non-existent data are dropped from further analysis (Little & Rubin, 2002). The main advantages of this method have to do with the simplicity in its implementation (i.e. does not require any special computation methods) and that it can be applied for any type of statistical analysis, allowing comparison between them because the sample base does not change from variable to variable (Enders, 2010).

On the other hand, the principal disadvantage of the CCD method is that it does not use all the possible information. More specifically, it reduces the sample size, which in turn reduces the statistical power and precision of the estimates. Thus, if not sensibly and properly applied, it can result into biased estimates (Little & Rubin, 2002). Note,

[70] See Little and Rubin (2002) and Särndal & Lundstrom (2005) for a more detailed discussion on this topic.
[71] The distribution of missingness and the pattern of the missing data cannot be analysed since there is practically no information on the lost units (i.e. total non-response).

however, that list-wise deletion normally produces unbiased regression slope estimates as long as the distribution of *missingness* is not a function of the outcome variable, which in this case will be assumed given the complete lack of information on missing units. Fortunately, since the amount of missing subjects is small, this assumption does not pose a significant threat to the validity of the estimates.

An alternative method that also seems logical to use is pair-wise deletion (PD), generally known as available case analysis. The main difference of this method with the previous one is that it uses all the available information by incorporating vectors of repeated measures of uneven length in the analysis (Nakai and Ke, 2011). In other words, it keeps as many cases as possible for each analysis in which the variables of interest are present. Therefore, it could be more efficient than the CCD method since the sample size is larger and also because it incorporates partial information.

Nevertheless, I have decided not to use pair-wise deletion because it does not allow comparison between different types of analysis. The reason is that the base sample changes from variable to variable according to the pattern of missing, which poses significant challenges in determining the sample size, degrees of freedom and combining different pieces of information (Enders, 2010). Since this thesis is based on different quantitative methods, and also due to the total non-response feature of the missing data, the PD method is no more useful in this case than the CCD alternative.

4.7 Constructing Necessary Variables for Data Analysis

I began the preliminary work with the data by creating a unique identifier of the households based on the existing identifiers of province, canton, parish, dwelling and household (i.e. database variables). This was done with the purpose of knowing the total number and composition of the households included in the panel-type database. More importantly, the households' unique identifier allows the subsequent construction of other necessary household-level variables. Note that since the demographic information of a

given family may vary from 2008 to 2014, the household identifiers were matched in such a way that the most recent information prevails (i.e. demographic data collected in 2014).

Next, I calculated age at survey date (i.e. 2008 and 2014) based on the moment on which the individuals were surveyed and the reported dates of birth. This may look as an easy task; however, it was necessary to correct possible age inconsistencies between both records, which added some complexity to this process. Note that this is a common aspect of the data cleaning and management process when working with survey data in general. In fact, many researchers have argued that it is essential to clean survey data prior to analysis to ensure accuracy and reliability (Barlas et *al*., 2016). The biggest concern has been about sub-optimal or less than accurate responses, which are direct consequence of a mistaken response (bias), an inattentive response (error), a recording error or an approximate response.

More specifically, and focusing on this thesis, the first problem in this respect was that some of the dates of birth declared in 2008 do not coincide with those declared in 2014. In order to correct for this misreporting error, the date of birth declared in 2008 was changed to the one declared in 2014, which is more recent and was obtained with better mechanisms of data collection. Nevertheless, there is a second and more important problem with this attempt to calculate simple ages of the individuals: there are some cases where the survey date in 2008 is not reported. Therefore, to correct this common problem of missing data, the survey date in 2008 was established as December 31 for those individuals without information. This simple change allows to obtain an approximate age of the person at that time.

Another variable that had to be generated was the “households' average level of educational attainment”. This variable is no more than the average rounded levels of education considering all the members of the family. The generated variable is rounded up with the sole purpose of maintaining the discrete nature of educational achievement. This practice is used in different academic and policy-making contexts in which an

attempt is made to reflect the general socioeconomic level of people taking into account the whole family (Horowitz & Weinhold, 1998; Crespo et *al.*, 2012; World Bank, 2016).

According to the 2008 Registro Social survey questionnaire, there are ten different categories of educational level, which are ranked from lowest to highest being "uneducated" the lowest and "post-graduated" the highest category. On the other hand, there are just nine educational attainment categories in 2014 because two of them (namely, categories 2 and 4), which are related to basic education, were grouped into one. Thus, for the average variables of "educational attainment" in 2008 and 2014 to be comparable, it was necessary to recode the categories in such a way that they coincide in both registers. In other words, the standardization of derived variables (i.e. the average degree of education) was crucial for the subsequent comparison.

Following a similar process, I constructed homogeneous indicators for the years 2008 and 2014 of household floor material (as a proxy of income level), school enrolment of the first-born child, school attendance of the first-born child, unemployment status, BDH and CDH beneficiary status, individual educational level, access to credit, ethnicity, and marital status. As a result, from the nearly 150 variables included in each Registro Social, I ended up with about 30 variables that are used in the analysis. Recall that the panel data includes individual level observations for two time-points (2008 and 2014). The following table summarizes and describes each one of these variables.

Table 10: Selected Variables from the Registro Social Panel-Database

Variable (Years)	Description	Type of variable
SELBEN index (2008 and 2014)	Overall welfare score calculated by MCDS using the 2008 methodology.	Continuous
Household Unique Identifier	Variable constructed by MCDS using the existing identifiers of province, canton, parish, dwelling and household.	Categorical (Discrete)

Household's average educational attainment (2008 and 2014)	Variable constructed as the rounded average of the educational attainment considering all family members.	Ordinal (Discrete)
Household floor material (2008 and 2014)	Homologated variable used as an income level proxy with eight different categories sorted from the lowest to the highest quality (i.e. soil, cane, wood, concrete or brick, marble, tile, plank or floating floor, other).	Ordinal (Discrete)
School enrolment of the first-born child (2008 and 2014)	Constructed dummy variable for the household's first-born child aged between 6 and 17 years old.	Dichotomous (Discrete)
School attendance of the first-born child (2008 and 2014)	Constructed dummy variable for the household's first-born child aged between 6 and 17 years old.	Dichotomous (Discrete)
Unemployment status (2008 and 2014)	Constructed dummy variable that takes a value equal to 1 if the person reported being unemployed and 0 otherwise.	Dichotomous (Discrete)
BDH Indicator (2008 and 2014)	Constructed dummy variable that takes a value equal to 1 if the person received BDH cash transfers a given year and 0 otherwise.	Dichotomous (Discrete)
CDH Indicator (2008 and 2014)	Constructed dummy variable that takes a value equal to 1 if the person received CDH cash transfers a given year and 0 otherwise.	Dichotomous (Discrete)
Individual educational level (2008 and 2014)	Homologated variable with nine different categories ranked from lowest to highest educational degree (i.e. none, literacy centre, primary, middle, secondary, post-secondary, high school, bachelor, post-graduate).	Ordinal (Discrete)
Province (2008 and 2014)	Homologated variable with twenty-four different categories corresponding to each one of the Ecuadorian provinces.	Categorical (Discrete)

Age (2008 and 2014)	Constructed variable based on the survey dates and the reported dates of birth.	Categorical (Discrete)
Marital Status (2008 and 2014)	Homologated variable with six different categories (i.e. free union, married, widowed, separated, divorced, single).	Categorical (Discrete)
Ethnicity (2008 and 2014)	Homologated variable with seven different categories of ethnic self-definition (i.e. indigenous, 'montubio', white, mestizo, black, mulatto, other).	Categorical (Discrete)
Access to credit (2008 and 2014)	Constructed dummy variable that takes a value equal to 1 if the person has received loans from commercial banks or moneylender sand 0 otherwise.	Dichotomous (Discrete)

Some additional remarks should be made regarding the variables selected and constructed for the analysis. First, the household-level indicators of school enrolment and school attendance were constructed considering exclusively the information of the first-born children aged between 6 and 17 years old (i.e. school-age children). In other words, when families have more than one child, only the first-born male or female is taken into account. As discussed next, since normally the first-born children are responsible for helping poor households financially, they are likely to interrupt their studies to achieve this purpose. For this reason, I consider it more useful to study the impact of cash transfers on the schooling situation of the household's older child than on a rounded average of all children in each family.

The claim that the first-born child of a poor household is the most exposed to replace (or accompany) the role of parents as breadwinners is supported by different empirical studies. In fact, birth order –as one of the family environment factors responsible for differences in child outcomes– is a recurrent topic in the economics and psychology literatures. The existing evidence on birth-order effects shows a clear and consistent

distinction between the developing world and the developed world: there are positive birth-order effects in developing countries, while there are negative birth-order effects in developed countries (De Haan et *al*., 2014). However, the specific mechanisms through which birth-order affects educational attainment are still unclear, and appear to be context-specific (Moshoeshoe, 2016).

For instance, applying a household fixed-effects estimation strategy on survey data from the Philippines, Ejrnæs and Pörtner (2004) find positive birth-order effects on educational attainment –measured by completed years of schooling of children aged 6 to 18 years old–, and that these effects are more pronounced in low-educated (or low income) families. More importantly, De Haan, Plug and Rosero (2014) use Ecuadorian survey data on infants (children under six year of age) and teenagers (between 12 and 18 years of age) to estimate long-term effects of birth-order on human capital. They find positive and consistent birth-order effects: first-born children lag behind in educational attainment from infancy to adolescence.

In addition, they show that first-born children are more likely to be involved in child labour than their younger siblings. According De Haan et *al*. (2014), these positive birth-order effects are in part explained by mothers' spending less quality time with first-borns, and breastfeeding them for a shorter period than later-born children. As expected, these birth-order effects are larger in poor (or low-educated) households, but they are negative in rich (or high-educated) households. Overall, evidence from developing countries largely supports the positive birth-order effects on human capital and, at least in the case of Ecuadorian poor households, the idea that first-born children are the most likely to interrupt their studies to contribute economically to their families through work.

Another observation to be made regarding the variables selected for the analysis has to do with the constructed BDH and CDH indicators. As explained above, both indicators are simply dummy variables that take values equal to 1 if the person has received monetary transfers and 0 otherwise for a given year. Note that by construction these variables do not indicate if a person was at that moment beneficiary of the

programmes, but if the person has *ever* received monetary transfers from a specific programme until the date of the survey. In fact, the questions asked by the Registro Social in 2014 were: have you been a beneficiary of the BDH? Have you received the CDH? Thus, the construction of the indicators for the year 2014 is straightforward. Nevertheless, some sort of difficulty arises because no questions were asked about the social welfare programmes in 2008.

In the case of the CDH programme, this issue is not important because until that time it had not yet been implemented at the national level and, although the first accumulated transfers were made precisely in 2008, there is not enough time for outcome effects to be produced. Therefore, the 2008 CDH indicator will be equal to zero in all cases. On the other hand, in order to construct the 2008 BDH indicator, it is necessary to assume that those who reported receiving cash transfers in 2014 were already beneficiaries in 2008. This seems quite likely since the official government data indicate that from 2008 to 2014 the number of beneficiary households of the programme increased by just about fifty thousand. This number is relatively small considering that in 2008 there were more than 1 million families receiving BDH cash transfers. However, it is necessary to emphasize that this assumption does not represent in any way a threat to the validity of the analysis, since the control group will be formed only by people who reported that they had *never* received the BDH.

Finally, it is worth mentioning that an attempt was made to generate a variable of frequency of attendance at health centres for infants. However, in 2008 these data are not recorded at all and in 2014 the variable only has 11 observations of children older than 5 years of age. The existence of such a large amount of missing data may reflect people's unwillingness to report information related to health centre attendance, perhaps for fear that this will have a negative impact on the direct benefits they receive from the government (i.e. the BDH programme's cash transfers). Therefore, the impact of the programmes on this specific variable cannot be analysed in this thesis.

Throughout this chapter, the main characteristics of the Registro Social process have been examined, as well as the methodology and composition of the SELBEN welfare index. In addition, it was mentioned that two different datasets were used to construct a panel-type database that is essential for the quantitative analysis. Given the distribution of individuals and the quality of the information, the CCD method has been used to deal with the missing data and perform the analysis. The last section discussed the generation and derivation of those variables necessary for the BDH and CDH impact evaluations. The specific evaluation design for the BDH programme is described in the next chapter.

CHAPTER 5

Bono de Desarrollo Humano Impact Evaluation Design and Measurement Models

5.1 Introduction

In this chapter, the necessary foundations for the different impact evaluations of the Bono de Desarrollo Humano (BDH) programme are established. The chapter commences with a theory-based approach to the BDH evaluation that allows the obtaining of an in-depth understanding of the workings of the programme's intervention. This is done through the inference and description of the Theories of Change (ToC) of the BDH together with a diagram depicting the design of the evaluation. This is followed by a thorough explanation of the sample selection process and the presentation of some descriptive statistics to explain the pre-treatment characteristics of the sample groups (i.e. BDH treatment and control groups), as well as to verify the assumption of "as if" random selection of beneficiaries (which is necessary for the validity of the natural experiment as discussed in Chapter 3). Finally, the last part of the chapter discusses the specific measurement models for data analysis. This section includes the identification strategies, the econometric

techniques to be used, and the specific regression models to be estimated as part of the BDH evaluation.

5.2 Theory of Change of the Bono de Desarrollo Humano

Theories of change (also referred to as programme theories) address the need to depict the cause and effect relationship between an intervention's activities and its intended results (i.e. the pathways to change) (Weiss, 1995). The use of a theory-based approach to evaluations allows us to consider the assumptions underlying causal chains from inputs to outputs. Therefore, the different impact evaluations in this thesis are structured around each programme's *Theory of Change* (ToC) to assess the causal logic of the interventions and determine whether all external factors affecting the results, impact, sustainability and up-scaling have been taken into account (UN, n.d.).

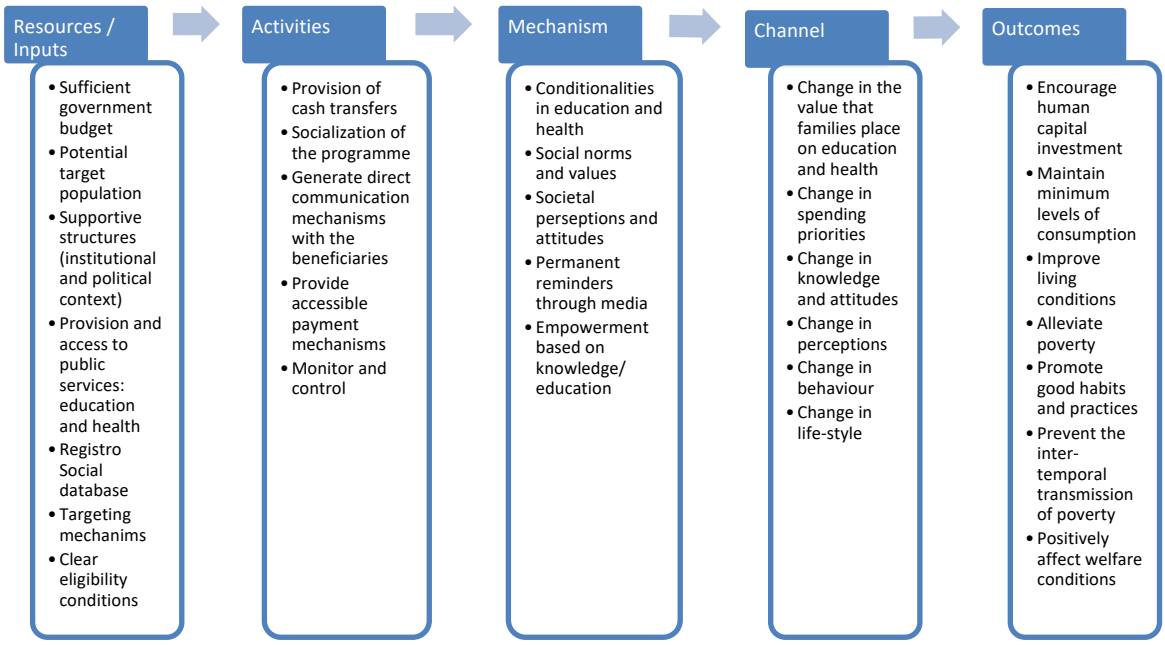
A ToC is essentially a methodology for planning, presenting and evaluating an intervention that is broadly used in different fields of research (Brest, 2010). Although there are different interpretations of this method, ToCs are generally understood as complete explanations and illustrations of how and why a desired result is expected to occur in a particular context. More specifically, it explains the process of change by outlining the causal linkages of an intervention, i.e., with its shorter-term, intermediate, and longer-term outcomes. This is achieved by representing the causal pathways from inputs to outcomes via intermediate states (Clark & Taplin, 2012).

In practice, the ToC methodology maps out or "fills in" the "missing middle" between what a programme or intervention does (i.e. the specific actions of the programme) and how these actions lead to expected results being obtained. This is done by first stating the objectives and expected outcomes, and then mapping backward from these to identify all the necessary preconditions that must be in place for the objectives to

be achieved (Brest, 2010; CTC, n.d.). This is not a purely intuitive process, but an elaborate and reasoned one. All the necessary information to describe the theories of change of the programme is obtained both from official government sources (VAMS, 2014; MIES, 2013; MCDS, 2013; Martinez & Rosero, 2007) and from international organizations, such as the Wold Bank, that have studied the BDH for several years and, therefore, have an in-depth knowledge of its internal functioning (Schady & Araujo, 2006).

There are endless variations in how ToCs are presented. In this case, I depict a diagram of the BDH programme dynamics of change (referred to as Outcomes Framework) and I complement it with a short narrative description. This diagram will guide the impact evaluations of the BDH cash transfer programme (see Figure 9). Once the effects of the BDH have been estimated in the next chapter, it will be important to get back to the theories of change described here to evaluate their relevance. More specifically, to compare the actual results obtained by the programme with the theoretical expected results. In other words, to contrast the levels of efficiency that the BDH programme presents in reality with those that, at least in theory, it should have.

Figure 9: Outcomes Framework of the BDH Programme



The aims and objectives of the Ecuadorian conditional cash transfer (CCT) programme, as well as its most relevant characteristics which will be also helpful for developing the programme's ToC, were already discussed in Chapter 2 (see page 64). The main goal is to help households overcome poverty by promoting investment in human capital (VAMS, 2014). Once the desired results of the programme have been established, it is necessary to determine which are the channels through which these results are expected to be obtained. The channels can be understood as *changes* in behaviour and attitudes, both in relation to the value given to education and health, and to the value that is given to the money available for spending. Moreover, the mechanisms used to promote these changes are varied, but undoubtedly the most important ones for this type of programme are the conditionalities attached to the cash transfers (Schady & Araujo, 2006).

As set out by the policy makers, discussed in Chapter 2, the activities carried out by the BDH programme to achieve the expected results are mainly: the provision of monthly cash transfers, the permanent search for means of payment of easy access for the beneficiaries, the continuous socialization of the programme seeking for the acceptance of the public, the establishment of direct communication mechanisms, and the attempt for an adequate monitoring and control (MIES, 2013; MCDS, 2013).

Finally, this process allows us to identify the necessary preconditions or resources that must be in place at least theoretically for the intervention to be effective. In case the impact evaluation indicates that the BDH programme is not effective, the explanation may be that some of these conditions are not being met. Apart from the availability of sufficient funds by the government, it is necessary to fully identify the target population by developing targeting mechanisms based on up-to-date socio-economic information (VAMS, 2014). In addition, the eligibility conditions that a poor household must meet to be chosen must be clear and easily determined by the programme administrators. Political and institutional frameworks are also important for a successful implementation of the BDH programme and the adequate provision of public services (such as education and health), which is a necessary and crucial element for compliance with conditionalities (Martinez & Rosero, 2007).

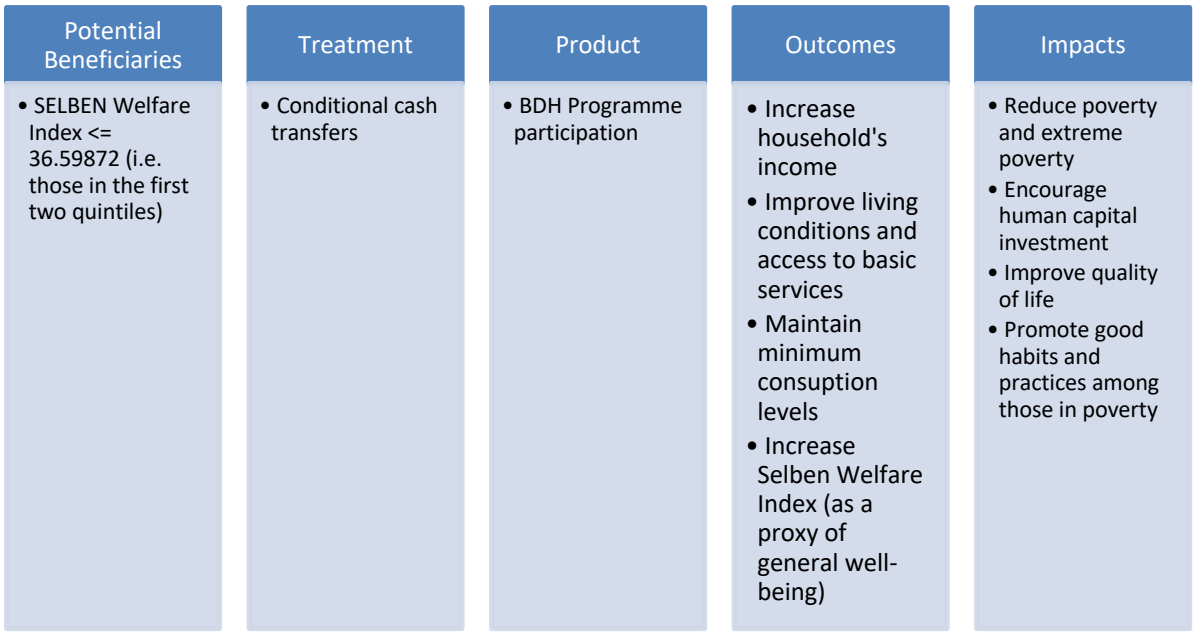
5.3 Evaluation Design

Now that the necessary pre-conditions (activities and resources) for the BDH programme to obtain the expected results have been determined, it is time to focus on the impact evaluation design. A rigorous evaluation of the programme starts from the development of a design/scheme that summarizes the logical and *observable* process through which the results are expected to be obtained. This scheme starts from the verifiable interventions on a specific group of people that in this case make up the target population.

Therefore, in order to make a precise estimation of the effects that the BDH programme had on well-being until 2014 (year in which eligibility conditions changed), it is first necessary to clearly identify in the Registro Social database the *potential beneficiaries* of the BDH cash transfers. Afterwards, it is important to describe the specific *treatment* given to some of those individuals and the *products* arising from this treatment. Finally, as part of the evaluation design, it is required to enumerate the specific *results* expected from the treatment; and ultimately determine the foreseeable *impact*s of the programme. These are the steps followed below.

As Figure 10 indicates, the potential beneficiaries of the BDH cash transfers are those poor households in the first two quintiles of the Welfare Index. The treatment itself is that the person gets a conditional monthly payment or not, so the product thereof is to participate in the BDH social assistance programme. The expected results, previously included in the Outcomes Framework, are described here in a more detailed way and then used to summarize the desired impacts of the programme.

Figure 10: Evaluation Design for the BDH Programme



5.4 Sample Selection Process

After describing the characteristics of the constructed panel-type database and specifying the selected variables for the analysis (in Chapter 4), it is time to focus on the BDH evaluation sample and its selection process. Recall that previously it was obtained from two different Registro Social datasets a total of 3,394,210 repeated individuals, which correspond in turn to 1,722,473 repeated households. Now, in order to build the BDH evaluation sample, only individuals with the following characteristics should be taken into account:

- ✓ Individuals in poverty situation according to the SELBEN index in 2008.
- ✓ Working-age adults (individuals who have at least 18 years old in 2008 and at most 65 years old in 2014)
- ✓ Individuals without disabilities (neither in 2008 nor in 2014)
- ✓ BDH beneficiaries and non-beneficiaries (of any social assistance programme) in 2014.

Note that the elderly and disabled groups do not necessarily fall into the category of poor individuals, and since I am interested in the BDH's independent effects on well-being among those in poverty, these groups should not be considered for the analysis. Moreover, those who report having participated in the CDH programme are not taken into account at this stage, because they constitute a different treatment group in a subsequent evaluation. This leaves us with a total of 1,994,965 individual observations.

In addition, it is necessary to further limit the sample to ensure that all individuals are unequivocally in a situation of poverty at the baseline moment of the study (namely in 2008). I use the SELBEN Welfare index to limit the sample only to those in poverty conditions prior to the intervention (i.e. potential BDH beneficiaries in 2008). Thus, only individuals in the first and second poverty quintiles are considered for the analysis. More specifically, if an individual has a welfare index equal to or lower than 33.5 points (i.e.

36.5 points minus a margin of 3 points that serves to reduce the possibility of taking into account cases of leakage), then she is considered a potential BDH beneficiary. This poverty threshold also ensures that the percentiles of well-being are similar between the pre-intervention treatment and control groups, which is desirable since both groups are of very different size (See Table 11). In fact, the control group is twice as large as the treatment group.[72] The resulting welfare percentiles are summarized in the following table:

Table 11: Welfare Percentiles at Baseline by Group

Welfare Index in 2008

	Percentiles by Group	
	Control[73]	BDH Treatment
1%	4.396	4.17
5%	8.203	7.844
10%	10.908	10.457
25%	16.301	15.65
50%	22.797	22.058
75%	28.173	27.328
90%	31.393	30.423
95%	32.482	31.562
99%	32.997	32.472
Smallest score	0	0
Largest score	33.5	33.5

[72] Note that this does not necessarily mean that the coverage of the BDH is low. It simply indicates that of the individuals with observations at two different timepoints, most have not received the BDH.
[73] Individuals categorized as living in poverty in 2008, but not receiving the BDH.

Observations	638,667	363,829
Mean	21.84802	21.14754
Std. Dev.	7.557734	7.407352

As a result of the sample selection process, the control group will be made up of those working-age individuals without disabilities who, being potential BDH beneficiaries in 2008 (i.e. in a situation of poverty), reported not having participated in any social assistance programme until 2014 (neither BDH nor CDH). On the other hand, the treatment group is made up of those people who, being potential beneficiaries in 2008, reported having participated of BDH programme in 2014. Table 12 shows the number of individuals belonging to each sample group.

Table 12: BDH Evaluation Sample Groups

Groups	Number of Individuals
Control	638,667
BDH Treatment	363,829
Missing	29,029
Total	1,031,525

5.5 Pre-treatment Characteristics of the Sample

For the natural experimental design to be valid, the assignment to the treatment and control groups should be entirely exogenous (or as-if random). Note that this premise constitutes the identifying assumption of the first proposed method of evaluation (i.e. natural experiment). In order to verify the assumed "as if" random selection of beneficiaries, it is necessary to perform a *base-line* comparison of the sample groups, which means to

compare them in terms of the observable pre-intervention characteristics. Table 13 presents descriptive statistics of the outcome variables by sample group in 2008.

Table 13: Pre-intervention Descriptive Statistics by Sample Group

	Control			BDH Treatment		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Welfare Index	21.848	7.557	638,667	21.147	7.407	363,829
Educational Attainment	2.519	0.990	638,667	2.387	1.014	363,829
Floor Material	3.964	1.136	638,667	3.896	1.165	363,829
School Enrolment	0.772	0.419	198,839	0.771	0.420	98,443
School Assistance	0.767	0.422	198,839	0.766	0.423	98,443
Unemployment	0.014	0.119	638,662	0.006	0.080	363,829

Overall, we can see from Table 13 that both sample groups are fairly *homogeneous* before the BDH intervention (i.e. in 2008). The seeming homogeneity of the sample groups suggests that there is no selection bias stemming from the BDH cash transfers assignment to poor individuals. In fact, all outcome variables have similar mean levels except for the "probability of being unemployed" indicator, whose value is slightly higher for individuals in the control group (see Figures 11 to 16). Therefore, we must be cautious when interpreting the natural experiment effect of the BDH on the probability of

unemployment. Figures 11-16 clearly support the idea that the selection of programme beneficiaries was as-if random.

Figure 11: Mean of Welfare Index in 2008

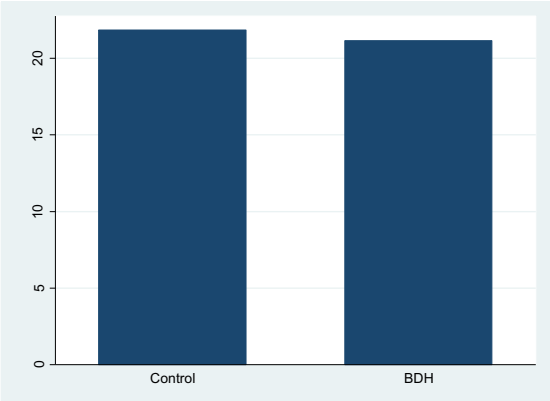


Figure 12: Mean of Households' Educational Attainment in 2008

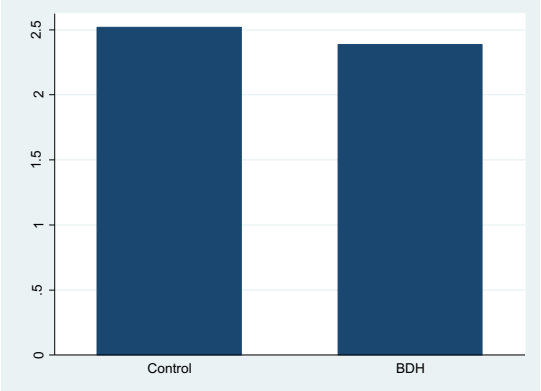


Figure 13: Mean of Households' Quality of Floor Material in 2008

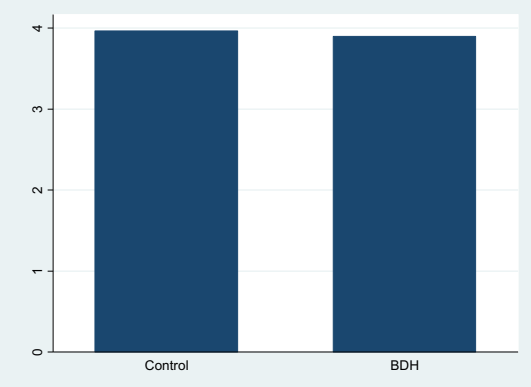


Figure 14: Mean Probability of School Enrolment in 2008 (First-born Child)

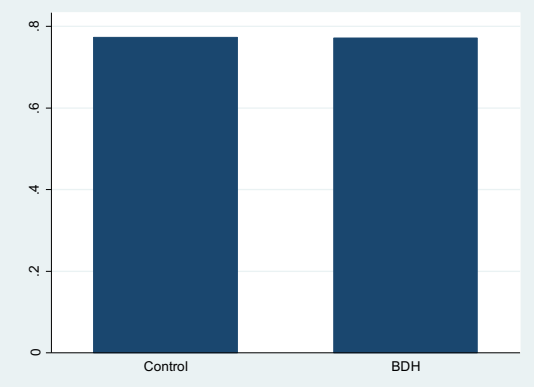


Figure 15: Mean Probability of School Assistance in 2008 (First-born Child)

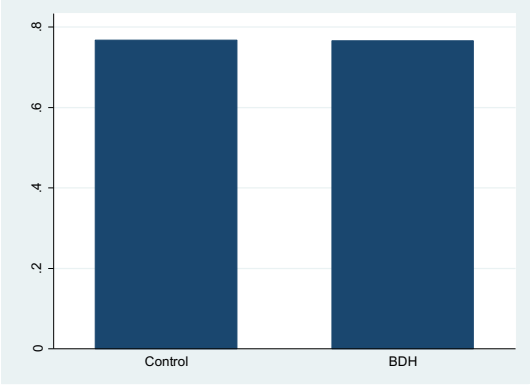
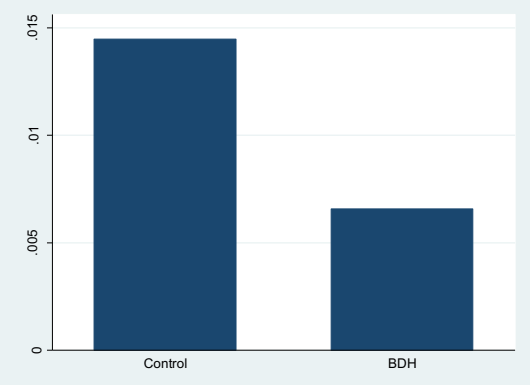


Figure 16: Mean Probability of Unemployment in 2008



5.6 Measurement Models for Data Analysis

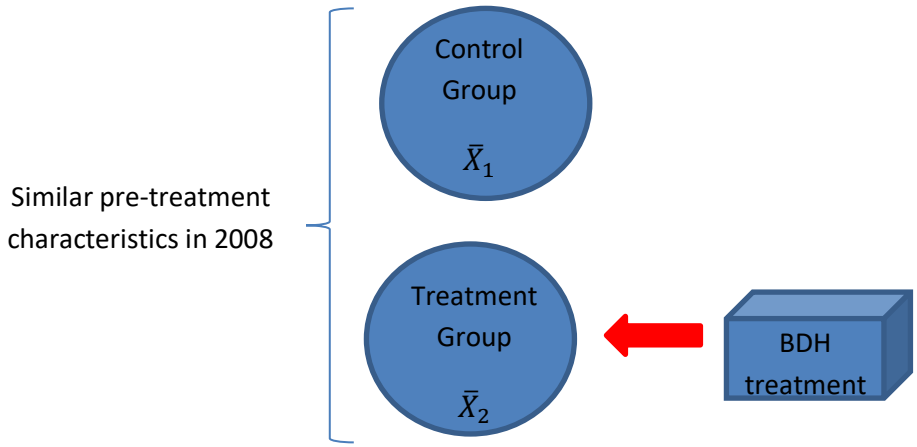
As discussed in Chapter 3, it is possible to get inferences of treatment effects through randomization. More specifically, if treatment assignment is independent of individual characteristics, then it is possible to estimate treatment effects with a simple difference-of-means test (Ho et *al*., 2007). Evidently, the same is true for natural experiments, whose only difference with randomized controlled trials is that researchers do not have the possibility to assign subjects to the 'treatment' and 'control' groups in advance. However, noticeable divergences in policy or practice offer the chance to study populations just like they had been part of a randomized or true experiment (McKenna & Morrison, 2009). The as-if random assignment avoids any omitted variable bias, even if the regression model does not include any controls. Therefore, in the case at hand, I could get valid estimates of the treatment effects using a simple regression model or a difference-of-means test if the assignment of BDH and CDH cash transfers was entirely random or as-if random (i.e. exogeneity condition), which fortunately can be argued to be the case.

The exogeneity condition is fulfilled since the government's budget for these programmes was insufficient to cover all households in the first and second poverty quintiles, so expansion of coverage has been rather gradual and based on an as-if random

selection of beneficiaries. I took advantage of two important elements characterizing cash transfer programmes in Ecuador. First, poor families that meet the requirements set by the BDH programme have been progressively and randomly selected to receive the cash transfers. Second, only beneficiaries of the BDH can switch to the CDH programme. These two facts constitute the basis for my natural experiment identification strategy.

The identifying assumption would be, therefore, that assignment to treatment and control groups is random (or "*as if*" random). Note that this assumption has already been verified by comparing the BDH treatment group with the control group in terms of their observable pre-intervention characteristics, which are reasonably similar and, therefore, support the randomization hypothesis. Once there are sufficient grounds to believe that treatment status is exogenous (or randomly assigned), estimation of *average treatment effects* (ATEs) is straightforward. Figure 17 illustrates graphically the natural experimental design used to perform this evaluation.

Figure 17: Natural Experimental Design (BDH)



As part of this natural experiment, I use two different econometric techniques: a simple difference-of-means test and regression model analysis.

5.6.1 Difference-of-means Test

For this evaluation, two independent groups of people in poverty and vulnerability conditions were chosen and, since treatment assignment was completely random, it is expected that they share similar observable characteristics before receiving the BDH cash transfers (i.e. in 2008).[74] Individuals who have been at some point randomly selected to be part of the BDH programme conform the treatment group and the others who have not been beneficiaries of any kind of social assistance are the control group. Then, a set of dependent variables is chosen on which I want to know if the treatment has had a statistically significant effect.

If the two previously homogeneous groups (beneficiaries and non-beneficiaries of the BDH) have similar means in the dependent variable after treatment (i.e. in 2014), this would mean that receiving the cash transfers has not made any difference in their current poverty situation.

Operationally, this approach consists of four steps: 1) state the hypotheses 2) formulate an analysis plan 3) analyse the sample data, and 4) interpret the results (Moore et *al*., 2009). First, every difference-of-means test requires stating a null hypothesis and an alternative hypothesis. These hypotheses must be stated in such a way that they are mutually exclusive. That is, if one of them is false, the other must be true; and vice versa. In this case, the null hypothesis states that there is no difference between the means of the treatment and control groups. Therefore, the hypotheses are formally stated in the following form:

$$H_0: \mu_1 = \mu_2 \ (i.e. the\ BDH\ has\ no\ effect)$$

$$H_a: \mu_1 \neq \mu_2 \ (i.e. the\ BDH\ has\ an\ effect)$$

[74] This was already examined in the previous section as part of the sample selection process.

Evidently, μ represents the means of the dependent variable for the treatment and control groups. From these hypotheses, I can effectively verify if the difference between the two means is due to the fact that the BDH treatment has had the expected effect or, on the contrary, the observed differences could be explained simply by chance.

Then, it is required to specify an analysis plan, which describes how to use the sample data in order to accept or reject the null hypothesis. The two main elements that make up the analysis plan are the test-method and the *significance level* (Moore et *al.*, 2009). Thus, this two-sample t-test will use a significance level equal to 0.05.

Finally, in order to analyse the sample data and interpret the results correctly, it is necessary to calculate the t-statistic and the P-value associated with it (Moore et *al.*, 2009). I have already indicated how the t-statistic will be calculated, while the P-value is defined as the probability of observing a sample statistic greater than the t-statistic. That is,

$$Pvalue = \Pr(|T| > |t|)$$

Therefore, if the P-value exceeds the significance level of 0.05, one cannot reject the null hypothesis that the means of the treatment and control groups are equal. On the other hand, one must reject the null hypothesis when the P-value is less than the significance level, which indicates that the BDH has a statistically significant effect on the dependent variable under study.

5.6.2 Multivariate Regression Model Analysis

In order to obtain the effects of the BDH programme on different outcome variables (Y), I estimate different versions of the following specific regression model:

$$Y_i = \alpha + \beta BDH_i + \sigma X_i + \varepsilon_i \,, \qquad (5)$$

Where Y is any of the outcomes under study, and β is the parameters of interest, which capture the causal effects of *BDH* (a dummy variable that equals 1 for the poor households receiving cash transfers from the BDH programme, and 0 otherwise) on the outcome under consideration. X is a vector of individual and household level characteristics, and ε is the error term.

Specifically, I estimate different versions of Equation (5) via ordinary least squares (OLS), probit or logit regression models, depending on the type of explained variable Y.[75] Note that for the estimation to be accurate it was necessary restrict the sample only to potential BDH beneficiaries who do not benefited from other social assistance programmes.

Although the treatment and control groups are quite homogeneous prior to the intervention, there may still be doubts about compliance with the exogeneity condition (i.e. "as if" random BDH treatment assignment). Therefore, I have decided to complement the analysis using a difference-in-difference quasi-experimental approach, which exploits the fact that the Registro Social process is repeated every 6 years.

5.6.3 Difference-in-Difference

In order to employ this method, it is necessary to take a step back and use the household unique identifier variable to convert the original constructed panel dataset from a wide format to a long format. This was not hard to do because all statistical program packages provide commands to convert data from wide to long format, and vice versa. More specifically, a reshaped database was created, which consists in adding all the variables in such a way that the 2008 and 2014 observations appear in a single column corresponding to each variable (e.g. SELBEN index, educational attainment, floor material, etc.).

[75] The outcome variables of interest could be continuous or categorical or ordinal, and different statistical models need to be used to analyse different types of explained variables.

Data organized in this type of format are also called *pooled* data. Andreß, Golsch, and Schmidt (2013) suggest that the reason for this name comes from the reflection that panel data can be thought of as consisting of many individual (unit-specific) time series that come together (pooled) in a single large data file. According to these authors, one could think of each panel wave as one cross-section, and of these different cross-sections as if they were grouped together (pooled) in one big file.

This can be very useful, since by viewing panel data as a collection of n unit-specific time series or, alternatively, as a collection of T cross-sections, one can understand why economists and political scientists also call panel data *pooled time-series cross-section (TSCS) data*. However, Andreß, Golsch and Schmidt (2013) are also very emphatic stating that, TSCS data should not be confused with data sets that pool *independent* cross-sections. Note that, contrary to panel data, the cross-sections are sampled independently from one another and, therefore, they do not include identical individuals.

In order to facilitate the subsequent analysis, the statistical package used in this thesis (STATA) automatically generates a pair of additional variables, which indicate the year of observation (i.e. panel wave) and were labelled as follows:

Variable	Description	Type of variable
TIME2008 Indicator	Constructed dummy variable that takes a value equal to 1 if the observation is from 2008 and 0 otherwise.	Dichotomous (Discrete)
TIME2014 Indicator	Constructed dummy variable that takes a value equal to 1 if the observation is from 2014 and 0 otherwise.	Dichotomous (Discrete)

Moreover, for methodological reasons, the following variables were created and added to the reshaped dataset as group indicators:

Variable	Description	Type of variable
BDH (Sometime)	Constructed dummy variable that takes a value equal to 1 if the individual reported having received BDH cash transfers at some point (i.e. 2008 or 2014) and 0 otherwise.	Dichotomous (Discrete)
CDH (Sometime)	Constructed dummy variable that takes a value equal to 1 if the individual reported having received CDH cash transfers at some point (i.e. 2008 or 2014) and 0 otherwise.	Dichotomous (Discrete)

In terms of the selected BDH diff-in-diff evaluation sample, it is made up of exactly the same households used in the preceding natural experimental models so that the results can be comparable. Once again, I restricted the sample according to the previously specified individual basic characteristics (see page 189), which are in function of the intended target population of the analysis (i.e. poor working-age people without disabilities).

As a result, the compared treatment and control groups are composed of the same individuals as in the previous methods, but with the important difference that this time I observe them at two different points of time (due to the employed TSCS data-set). Therefore, the number of observations would have doubled if it were not for the few but still existing missing observations in 2008. Some researchers argue that, since pooling increases the number of cases, it also does so with the statistical power of the analysis (Andreß et *al.*, 2013). Instead of n units, they say, the data includes $N = n * T$ "cases",

where *T* is the number of panel waves. This argument is often used in economic, sociological, and political science research.

As in the difference-of-means test and in the natural experimental models, the control group is made up of those individuals who did not receive cash transfers from any programme (despite being potential beneficiaries); while the treatment group is composed of those who were at some point beneficiaries exclusively of the BDH programme. Given that the pooled data includes information of two different panel waves –both for the BDH treatment and control groups– it is possible to use the diff-in-diff method to isolate the externalities correcting for any potential bias (namely, potential endogeneity problems caused by omitted variables) in order to calculate in a more accurate way the impact of the BDH treatment among poor households.

By using the difference-in-differences method, it can be safely stated –with more certainty and less rigorous assumptions– that the estimated effects are effectively explained by the BDH treatment. The diff-in-diff estimation is by itself statistically rigorous enough to infer causality in the studied relationships, even in the absence of the so-called randomization assumption, which was essential for the validity of the natural experiment (Stuart et *al*., 2014). Actually, the main difference between these two quantitative evaluation methods is that in natural experiments control and treatment groups are assumed to be analogous before the intervention and evolved similarly in the absence of it. By using a difference-in-differences strategy, these assumptions do not need to hold priori because difference between groups are removed by subtracting the change in means of control group from the change in means of treatment group over the time period considered in the study (World Bank, 2013).

Thus, the small pre-treatment differences between treatment and control groups that were described in this chapter would not be a major concern for the diff-in-diff estimation of the BDH effects. Although to guarantee parallel trends over time, it is still desirable to have groups as similar as possible. All the necessary assumptions for the validity of the diff-in-diff model used in this thesis will be further developed in the next

chapter, which analyses the impact evaluation results obtained for the BDH and its statistical significance (see page 233).

In order to evaluate the effect of the BDH programme on the outcome variables, first I construct an indicator of whether an observation belongs to a "sometime" treated individual and was obtained in 2014. Since I have two groups (treatment and control) and two years (2008 and 2014), this indicator is constructed based on the interaction of an observation's time and group indicators. However, note that it is important to include time and group indicators as separate control variables in the regressions, because they are likely to influence the outcome variables directly and independently of whether an individual was affected by the BDH cash transfers. In other words, I need to account for group specific characteristics and trends in time that could be affecting the estimated results.

Formally, I would have a BDH treatment ($j = 1$) and a comparison ($j = 0$) group for both the before ($t = 0$) and after ($t = 1$) periods of time. Thus, the estimated regression model is the following:

$$Y_{it}^{j} = \beta_0 + \beta_1 TIME2014_t + \beta_2 BDH^{j} + \delta(TIME2014 * BDH)_j^t + \beta_3 X_{it}^{j} + \varepsilon_{it}^{j} \qquad (6)$$

Where Y_{it}^{j} is the level of the outcome variable of a given individual i in group j at time t. BDH^{j} is a dummy variable that takes a value equal to 1, if the observed person belongs to the treatment group, and takes value equal to 0, if the person is in the control group. $TIME2014_t$ is another dummy variable, which takes a value equal to 1 in the post-treatment period; and 0 otherwise. The diff-in-diff estimator is δ, the coefficient of the interaction between BDH^{j} and $TIME2014_t$. Note that this interaction term is also a dummy variable, which takes a value equal to 1, only for the BDH treatment group in the post-treatment period. Lastly, X_{it}^{j} is a set of control variables and ε_{it}^{j} is the error term.

The other terms in equation (6) are the coefficients $(\beta_0, \beta_1, \beta_2, \delta, \beta_3)$ of the diff-in-diff regression model, which help in deriving different effects for the treatment and control groups. Thus, this regression is useful in deriving the BDH effects, namely, whether this specific programme made a difference to Y_{ijt} in the treatment group after the intervention. Finally, recall that the employed regression models (OLS, probit or logit regression models) will depend on the type of explained variable Y (continuous, categorical ordinal or dichotomous).

The different measurement models and econometric techniques presented in this chapter constitute an essential part of the BDH impact evaluation design. Now that the foundations for the analysis have been clearly established –taking into account the Theory of Change of the programme and the sample selection process–, it is time to present the obtained results. The following chapter describes the estimated effects of the BDH programme on the different outcome variables.

CHAPTER 6
The Unexpected Effects of the Bono de Desarrollo Humano

6.1 Introduction

In this chapter, the results of the different impact evaluations of the Bono de Desarrollo Humano (BDH) programme are presented and discussed. More specifically, I will focus on analysing, through different statistical and econometric methods, the effects of the BDH programme on different measures of non-monetary poverty and human capital, which include the level of well-being, educational attainment, school enrolment, housing conditions and employment status. This chapter begins by analysing the post-treatment characteristics of the sample. This is followed by the results of the natural experimental method, which are obtained by two different econometric techniques. Finally, the last part of the chapter discusses the results obtained by the difference-in-differences method.

The main purpose of these findings is to help propose a reasoned explanation of the role played in the fight against poverty by the main social assistance programme in

Ecuador and, of course, to answer that research question posed at the beginning of this thesis about the true effectiveness of the BDH conditional cash transfers. Understanding that this represents a necessary step to better comprehend the different effects that social assistance policies and practices have on different poverty measures (monetary and non-monetary). In doing so, an attempt is made to fill the gap that evidently exists in the evaluative literature on these issues, implementing alternative methods of quantitative research and exploiting the availability of more recent data, for the benefit of the academic debate and policymakers.

6.2 Post-treatment Characteristics of the Sample

Before focusing on the results obtained from the regressions, it is useful to begin by showing the end-line descriptive statistics of the outcome variables by sample group (see Table 14). In this way, we can know if there has been a change in these variables over time and, more important, if there are actual differences between the treatment and control groups. Table 14 clearly shows that from 2008 to 2014 there has been a considerable improvement in all the outcome variables analysed. For example, the mean of the control (treatment) group's welfare index goes from 21.84 (21.14) to 32.51 (30.64) points. This means that after 6 years the welfare level of the sample increased by about 50 percent, which is consistent with different studies that indicate that in the last few years Ecuador has had a good performance in the fight against poverty (BCE, 2012; INEC, 2012).

Similarly, the results show improvements –albeit smaller– in educational attainment, the quality of the home floor material, school enrolment, school attendance, and unemployment. However, it is important to note that these initial results tell us nothing about the different factors that have influenced the change and the improvement over time in the studied outcome variables. The application of the difference-in-differences method will be useful in this sense as will be seen later.

Table 14: Post-intervention Descriptive Statistics by Sample Group

	Control			BDH Treatment		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Welfare Index	32.519	11.360	638,667	30.648	10.891	363,829
Educational Attainment	2.714	1.258	638,667	2.452	1.191	363,829
Floor Material	4.514	1.212	638,667	4.40	1.247	363,829
School Enrolment	0.805	0.395	86,220	0.791	0.406	50,549
School Assistance	0.780	0.414	86,220	0.765	0.423	50,549
Unemployment	0.015	0.122	638,667	0.006	0.082	363,829

Moreover, it should be noted from Table 14 that in 2014 (i.e. after the BDH programme intervention) the treatment and control groups are *no* longer homogeneous and, in fact, there are considerable differences in some of the variables. Surprisingly, the end-line descriptive statistics indicate that the control group is in a better situation compared to the treatment group with respect to almost all outcome variables. For instance, the average welfare level in the control group is of 32.51 index points, while the BDH beneficiaries present an average welfare of 30.64 points. Therefore, under the assumption that the allocation of transfers is random among poor families, these initial results suggest that the BDH has not had a positive effect on the level of well-being and the human capital accumulation of the households. Actually, the impact seems to be slightly *negative* in almost all cases. Nevertheless, the magnitude and statistical significance of the BDH effects will be determined by the regression analysis that follows.

Remarkably, only the probability of being unemployed is greater in the control group with almost 1 perceptual point over the treatment group. Therefore, the BDH apparently has had a positive effect on the labour supply among beneficiaries. However, we must keep in mind that this was already happening in 2008 and, in fact, the difference between the groups is very small. Therefore, in this specific variable, we cannot (at least for the moment) attribute to the BDH effects the fact that if a person belongs to the treatment group she is less likely to be unemployed. Of course, this will be statistically determined later by the method of difference-in- differences.

6.3 Natural Experimental Results

As part of the natural experimental design, I use two different econometric techniques: a simple difference-of-means test (two-sample t test with equal variances) and regression model analysis. The results obtained from the difference of means tests for the outcome variables in 2014 are summarized below:

Table 15: Difference-of-Means Tests (Control Group vs. BDH Treatment Group)

	Group									
	Control			Treatment (BDH)			t-test for Equality of Means			
	Mean	SD	n	Mean	SD	n	95% CI for Mean Difference	t	df	Sig. (2-tailed)
Welfare Index	32.51	11.36	638,667	30.648	10.891	363,829	1.826, 1.917	80.510	1.0e+06	0.00
Educational Attainment	2.714	1.258	638,667	2.452	1.191	363,829	0.256, 0.266	101.99	1.0e+06	0.00
Floor Material	4.514	1.212	638,667	4.400	1.247	363,829	0.108, 0.118	44.737	1.0e+06	0.00

School Enrolment	0.805	0.395	86,220	0.791	0.406	50,549	0.010, 0.018	6.515	136,767	0.00
School Attendance	0.780	0.414	86,220	0.765	0.423	50,549	0.010, 0.019	6.478	136,767	0.00
Unemployment	0.015	0.122	638,667	0.006	0.082	363,829	0.007, 0.008	36.907	1.0e+06	0.00

* p-value < 0.05. Ho: diff = 0.

Table 15 shows that there is a statistically significant mean difference between the control group and BDH treatment group in all the outcome variables. At a confidence level of 95 percent, the p-values of these t-tests are 0.00 (which does not surpass the significance level of 0.05), and therefore, the null hypotheses of equality of means *can* be rejected.[76] Surprisingly, these results clearly suggest that benefiting from BDH cash transfer has no positive effects on the level of well-being, educational attainment, quality of the home floor material, school enrolment and school attendance. In fact, the effects appear to be negative –although small– except for the effect on the probability of being unemployed.

For instance, BDH beneficiaries tend to have a slightly smaller welfare index than those in the control group. According to the 95 percent confidence interval for the estimated mean difference, this type of treatment decreases individual scores on the welfare index to a maximum of 1.91 points, that is, the BDH cash transfers worsen beneficiaries' poverty situation in around two points. Similarly, the BDH treatment decreases the probability of school enrolment and school assistance of the first-born child in around 1 percentage point. On the other hand, the BDH programme seems to improve the labour supply of the beneficiaries. More specifically, the probability of being

[76] The two-tailed p-value is computed using the t distribution. It represents the probability of observing a greater absolute vale of t under the null hypothesis of equality of means, i.e. [Pr(|T|>|t|)]. If the p-value is less than the pre-specified level of significance (normally 0.05 or 0.01) it can be concluded that the mean difference between treatment and control groups is statistically significantly different from zero. On the contrary, if the p-value is greater than 0.05, we conclude that the mean difference is *not* statistically significantly different from zero.

unemployed decreases to a maximum of 0.8 percentage points among cash transfers recipients.

In general, these t-tests reaffirm that the control group is in a better situation than the BDH treatment group after six years of intervention. However, in order to determine the *independent* effect that the BDH has had on the outcome variables (once different controls are included), it is necessary to carry out a more rigorous analysis through various regression models. These results are presented in Tables 16-20.

6.3.1 The Effects of the Bono de Desarrollo Humano on the Welfare Index

The linear regression estimates of the impact of the BDH cash transfers on the SELBEN welfare index, as a measure of poverty reduction among the poorest households, are presented in Table 16. All the regressions in this table control for province of residence, marriage status, and self-defined ethnicity fixed-effects. In the first specification, the household's educational attainment, the quality of floor material and the unemployment indicator are not considered.

The first column indicates that with the mere inclusion of control dummies, there are sizeable negative effects of the BDH on welfare. Specifically, the BDH programme decreases the SELBEN welfare index by about 2.1 points, and the estimates are highly statistically significant. Note that these effects represent a relatively important decrease in welfare, since the mean welfare of the sample in 2008 (i.e. before the intervention) was 21 index points and the difference between the lowest (i.e. the 1 percent poorest) and highest (i.e. the 1 percent less poor) percentiles was about 28 index points.

Besides, the coefficient on the BDH indicator is relatively precisely estimated. Although not included in Table 16, the 95 percent confidence interval for the effect of the BDH cash transfers on the welfare index ranges from -2.22 to -2.14. The BDH coefficient

is somewhat different when the household's average educational attainment is included in column (2). The estimate is still negative and significant, indicating that the BDH effect is not cancelled by the positive effect that each completed educational degree has on welfare. However, the impact of the BDH is smaller (-1.45), suggesting that some part of the negative effects can be linked to the direct effect that the BDH has on educational attainment (see Table 17).

In column (3), when the household's quality of floor material –a proxy of the household's income level– is included as an explanatory variable for welfare, the BDH coefficient is again consistently negative and significant. However, in this specification, the coefficient on the BDH indicator is once again reduced in magnitude. Once the quality of floor material is included in the regression, the 95 percent confidence interval for the impact of the BDH cash transfers on the welfare index ranges from -1.19 to -1.12. This implies that a considerable part of the effects of the BDH on welfare may be due to a direct negative effect of the BDH programme on the household's income level (see Table 18).

The results of the BDH coefficient did not change significantly when I gradually included controls for individuals' unemployment situation (column 4) and age (column 5). In the fourth specification, the BDH estimate is again negative, highly statistically significant and slightly smaller. Specifically, with a confidence level of 95 percent, the coefficient lies between -1.19 and -1.11. Similarly, column (5) shows that the estimated effect of BDH cash transfers on welfare remains practically unchanged when the age of individuals is included in the regression.

As before, the last results suggest that a small part of the BDH effects on welfare may be driven by the fact that cash transfers beneficiaries are less likely to be unemployed (see Table 20). However, the positive relationship that has been found between unemployment and well-being is somewhat disturbing. It is possible to explain this relationship by arguing that being unemployed gives people the possibility of performing other activities related to welfare, such as improving housing conditions. Another possible

explanation is that only people who reach a certain level of well-being can afford to be unemployed for long periods (i.e. reverse causality issues).

Altogether, the linear regression results in Table 16 indicate that the BDH conditional cash transfers did affect individuals' well-being, but the opposite way than expected. The obtained results suggest that the BDH has had a significant and negative impact on well-being among those in poverty. As different control variables are added to the least squares regressions, the magnitude of the BDH effect on the welfare index is gradually reduced until it reaches -1.1 points.

Moreover, and as expected, the independent effects of household's educational attainment, quality of floor material, and individual's age (a labour experience proxy) on welfare are positive and significant. Since these predictors are ordinal independent variables, I decided to treat them as if they were continuous. This incorporates into the model the fact that the categories are ordered, but also assumes that equal changes in X mean equal changes in Y. Thus, the increase of a category in the average level of education of the family has an effect of 2.79 points on the welfare index. Similarly, the effect of floor material is interpreted as the difference in the predicted value of the welfare index for each-one unit difference in the predictor, if all other independent variables remain constant. Note that a one unit difference represents switching from one category to the other. Therefore, the average difference in welfare between floor material categories is 3.93 index points. While the effect on the welfare index of each additional year of age is 0.11.

Finally, note that in my OLS regression analysis, I found R-squared values ranging from 18 percent in the first specification to 45 percent in the full specification (see last row of Table 16). The R-squared, or coefficient of determination, represents the proportion of the variance in the dependent variable that is *predictable* from the independent variable(s) (Everitt & Skrondal, 2010). The concern that usually arises –when analysing the coefficient of determination– is whether the R-square is sufficiently high and, unfortunately, there is not easy answer to this question because it totally depends on

the context of each model (MacDonnell, 2010). The only possible answer is that the estimated R-square must equal the percentage of outcome variable variation that is explained by the specific linear model, no more and no less. Therefore, the key question is actually whether the estimated coefficients of determination are adequate given the major objectives of the linear regression models.

For instance, if the primary objective would be to produce predictions of the dependent variable that are reasonably precise, then the estimated R-squared values would become a concern (Frost, 2014).[77] The relatively low R-squared values obtained in the analysis indicate that the model has some "prediction error" and, thus, that making projections of the SELBEN welfare index would be very difficult –given the factors that I have included in the regressions (which seems understandable especially in Column 1 when only the BDH indicator is included). In fact, in the initial specifications, it is evident that most of the variation in the outcome variable should be explained by unobserved factors.

However, making predictions is not at all the goal pursued by the linear and non-linear models in this thesis, but rather determining which predictors are statistically significant and how changes in the predictors relate to changes in the outcome variables. When the main objective is to analyse the relationship between the predictors and the response variable, the R-squared value is almost totally irrelevant (Frost, 2014). In other words, if the regression model is correctly specified, the R-squared doesn't affect how you interpret the estimated results. The interpretation of the coefficients and their statistical significance is the same regardless of whether the coefficient of determination value is 10 percent or 95 per cent. Therefore, in this case, the R-squared doesn't need to be any particular value (low or high) to validate the employed models and the obtained estimates.

Moreover, it is important to consider that in some studies, it is entirely expected that R-squared values will be low. More specifically, any field of research that attempts

[77] While high R-squared values are required for precise predictions, they are not sufficient by themselves (Frost, 2013).

to model human behaviour, such as psychology, normally presents estimates with R-squared values lower than 50 percent (Chatterjee et *al*., 2014; Frost, 2013). The reason is that people are simply harder to predict than, for example, economic processes. In this thesis, though the R-squared values are low, the F values of the models are significant. This denotes that together the explanatory variables used in the models have significant explanatory power.

6.3.2 The Effects of the Bono de Desarrollo Humano on Educational Attainment and Housing Conditions

In order to determine which welfare components have been the most negatively affected by the BDH, I estimate the independent effects on other outcome variables considered in this thesis. As statistical modelling theory requires, depending on the type of dependent variable (i.e. ordinal categorical or dichotomous) different regression models are used in the analysis (i.e. ordered logistic or probit models). Specifically, in this section of results, I use Registro Social data to look at the impacts of the BDH on the household's average level of education and income. The data set contains questions about self-rated educational achievement and housing conditions. I use this information to create some ordinal categorical measures of human capital and household income, and I run regressions with these as dependent variables. In general, ordinal discrete outcomes take on three or more values that can be rank ordered, but an assumption of equal-sized intervals between the response options is usually not warranted. This type of dependent variables is analysed via ordered logistic regression models (also known as ordered logit or proportional odds models), which predict the likelihood of an individual appearing in each successively higher category of the outcome variable.

Tables 17 and 18 summarize the ordered logistic regression results for the effects of the BDH cash transfers on households' education and floor material. Households' education is a categorical response variable describing the self-reported educational

attainment of families (rounded average of all its members) into nine categories ranked from lowest to highest educational degree: 1 no education, 2 literacy centres, 3 primary education, 4 middle education, 5 secondary education, 6 post-secondary education, 7 high-school education, 8 bachelor degree, and 9 post-graduate. Meanwhile, the floor material response variable specifically describes its self-reported quality into eight different categories classified from the lowest to the highest level: 1 soil, 2 cane, 3 wood, 4 concrete or brick, 5 marble, 6 tile, 7 plank or floating floor, and 8 other. All specifications in the tables control for reported province of residence, marriage status and ethnicity. In addition, different control variables –including an unemployment indicator and individual's age– are progressively included in the regressions. Note that the first half of each column interprets the coefficients in terms of ordered log-odds (logits) and the second half interprets the coefficients in terms of proportional odds. Both interpretations are explained in detail below.

According to the results in Table 17, the BDH cash transfers have *negative* and statistically very significant effects on the households' educational attainment. In the first specification (column 1), with few control variables, the ordered log-odds (logit) coefficient on the BDH indicator is -0.3577. The standard interpretation of an ordered logit coefficient is that for a one unit increase in a given predictor, the outcome variable level is expected to change by its respective regression coefficient in the ordered log-odds scale, while the other variables in the model are held constant. For example, -0.3577 is the ordered log-odds estimate of comparing those affected by the BDH to those not affected on expected educational attainment. Therefore, the ordered log-odds for BDH-affected individuals of being in a higher education category are 0.3577 less than individuals not affected, when the other variables are held constant.

The second half of column (1) displays the coefficients in terms of proportional odds ratios, which allow for the comparison of individuals in groups greater than a given level with individuals in groups less than a given level.[78] The general interpretation would

[78] Proportional odds ratios are obtained by exponentiating the ordered logit coefficients ($e^{coef.}$).

be that for a one unit change in the predictor variable, the odds for cases in a group that is greater than *k* versus less than or equal to *k* are the proportional odds times larger. In this case, 0.69 is the estimated proportional odds ratio of comparing individuals affected by the BDH to those not affected on education attainment of the household. This coefficient indicates, for instance, that for BDH-affected individuals, the odds of a higher than secondary education versus the rest of the categories combined are 0.69 times lower than for not-affected individuals, given that the other variables are held constant in the model.

The impact of the BDH programme is still negative and statistically significant when floor material is controlled in column (2). Both, the ordered log-odds estimate and the proportional odds ratio, remain practically unchanged (although they slightly decrease). Specifically, the ordered log-odds for BDH-beneficiary individuals of being in a higher education category are -0.3369 less than for those not affected, given that the other variables are held constant. In terms of proportional odds ratio, the odds of a higher than secondary education versus the rest categories combined are 0.71 times lower for BDH-affected individuals. Apparently, some small part of the negative impact of the BDH was due to lower income levels faced by programme beneficiaries, since the findings suggest a positive and significant effect of the quality of floor material (income proxy) on educational attainment.

Similarly, when the unemployment indicator is also included in the third specification (column 3), the negative impact of the BDH on education remains almost unaffected. Meanwhile, the unemployment indicator estimates are positive and significant, suggesting that being unemployed has a positive independent effect on the educational attainment of the households. Therefore, these results indicate that the BDH effects are not driven for the most part by possible differences in the probability of unemployment between beneficiaries and non-beneficiaries of the cash transfers. In addition, although differences in the probability of unemployment are likely to be present (because the BDH effect slightly decreases), they should be relatively small in favour of the beneficiaries of the programme (i.e. less probability of unemployment for them).

Finally, when individuals' age is controlled in the full specification (column 4), the negative effect of the BDH slightly decreases again (becomes less negative). To be more precise the ordered log-odds for BDH beneficiaries of being in a higher education category are -0.3171 less than for those who do not receive the transfers, given that the other variables are held constant. In terms of proportional odds ratio, the odds of a higher than secondary education versus the rest categories combined are 0.72 times lower for BDH-affected individuals. These results suggest that a small part of the BDH effect was due to relatively higher ages of individuals in the treatment group, since there is a negative and significant impact of individuals' age on their households' average educational attainment.

Overall, the results in Table 17 indicate that the BDH has a significant negative effect on the average level of education of families. The magnitude of the effect is considerable, since the ordered logit probability that the BDH beneficiaries are in a higher category of instruction level is about -0.33 less than the individuals in the control group, given that the other variables in the model are held constant. One possible explanation for these findings is that the BDH recipients have less incentives to study and thus improve their skills and future income levels. Besides, it appears that the existing control mechanisms of conditionalities are not effective enough to ensure that families invest in their human capital. Meanwhile, the households' quality of floor material and the unemployment indicator have positive impacts on educational attainment, clearly reflecting that higher levels of income and more time available improve the probability of achieving a higher educational level.

Moreover, the results reported in Table 18 show a significant negative effect of the BDH programme on the household's quality of floor material. Controlling only for reported province, marriage status and ethnicity, the estimated ordered logit coefficient on the BDH indicator is sizeable at -0.2017 log-odds units and statistically significant at the 1 percent level. The same coefficient in terms of proportional odds ratio indicates that the odds of a better floor than concrete versus the rest categories combined are 0.81 times lower for BDH-affected individuals. Column (2) shows that including educational

attainment as a regressor leads to an important reduction in the impact of the BDH (i.e. the coefficient becomes less negative). However, the ordered logit estimate for the effect of the BDH is still negative and significant at -0.1380 log-odds units, while its respective odds ratio is 0.87. Apparently, a significant portion of the negative BDH impact on floor material (household's income) was due to lower educational levels obtained by programme beneficiaries. These results are consistent with those that demonstrated previously (see Table 17) that the BDH has a significant negative effect on the average level of education.

Additionally, the BDH coefficients are very similar when the unemployment indicator and individual's age are taken into account progressively in the third and fourth specifications (columns 3 and 4). In both cases, the ordered log-odds of being in a higher floor material category are again 0.13 lower for BDH affected individuals than for those not affected, given that the other variables are held constant. In terms of proportional odds ratio, the odds of a better floor than concrete versus the rest categories combined are, therefore, 0.87 times lower for BDH beneficiaries. The rest of the coefficients in Table 18 indicate that being unemployed has a positive independent effect on the quality of floor material, while there is a negative impact of individual's age. Thus, these results indicate that the BDH effects are not driven for the most part by possible differences in the probability of unemployment and age between beneficiaries and non-beneficiaries of the programme.

In summary, the results in Table 18 show that the BDH programme has a significant negative effect on the household's quality of floor material, which is used as a proxy for the income level. The magnitude of the effect is relatively small (-0.13 log-odds units) considering that the categorical ordinal outcome variable of floor material has eight different categories. On the other hand, the educational attainment of the family and the unemployment indicator have positive impacts on the floor material, reflecting a positive relationship between human capital and (perhaps) the available leisure time with the level of well-being. In the case of the unemployment indicator, the nature of this relationship is still unclear. As noted above, it is difficult to explain the mechanisms through which being

unemployed may have such positive impact on the household's floor material, which is a common component of well-being. However, there may be a relationship between unemployment and well-being beyond the effect of contemporary income. A plausible explanation is related to the more available time the unemployed may have to carry out other activities, such as improving the housing conditions.

In fact, some recent research suggests that the unemployed can spend more time in pleasurable leisure activities, hence increasing effective well-being (Powdthavee, 2012; Knabe et *al*., 2010).[79] This points to the question whether unemployment impacts differently on different welfare measures. To answer this question, quantile regressions have recently been introduced to study subjective well-being (Binder & Freytag, 2012; Binder & Coad, 2011) and they provide evidence for considerable *heterogeneity* in the impact of unemployment on the well-being distribution. In any case, it is important to note that the results presented in Table 18 are consistent with those previously found in this thesis suggesting a positive relationship between unemployment and well-being.

6.3.3 The Effects of the Bono de Desarrollo Humano on Human Capital of the First-born Children

A plausible explanation for the absence of positive effects of the BDH programme on welfare may be that, in this specific case, well-being differences are not yet reflected in the household's average educational attainment and quality of floor material, which are two of the most important components of the SELBEN welfare index used in this analysis. However, there may be significant differences in the investment of human capital between beneficiary and non-beneficiary families, which for the moment are reflected only in school enrolment and school attendance of the first-born children. As mentioned above, they are the most exposed to replace (or accompany) the role of parents as the

[79] However, it runs counter to a considerable amount of research on long-term unemployment and health (Romeu, 2006).

breadwinner. For example, it could be the case that children who were affected by the BDH cash transfers have more probabilities of being enrolled and attending school, but this does not reflect yet in the household's average level of human capital. Thus, in order to test this hypothesis, I use the Registro Social data to construct indicators of school enrolment and school attendance for the first-born at the cost of fewer observations because only a portion of the individuals in the sample have children.

The data set contains information about school enrolment and school attendance at the time of the interview. This allows me to construct dummies with the purpose of using them as dependent variables in different regressions. Due to the binary nature of the outcome variables, I use probit regression models in the analysis. These models calculate the predicted probability of being enrolled and attending school on the basis of the regressors used. They do so using the cumulative distribution function of the standard normal. The probit regression results for the effects of BDH cash transfers on school enrolment and attendance status of the first-born children are presented in Table 19. Once more, all the specifications in the table control for individual's province of residence, marriage status and ethnicity fixed-effects. Meanwhile, household's average educational attainment, quality of the floor material, unemployment status and age are progressively included in the specifications as regressors.

The first half of Table 19 presents the BDH effects on the probability that the first-born children are enrolled in school. In column (1), a dummy for being enrolled in school is regressed on the BDH indicator and on a set of control dummies. The initial results show a significant negative effect of the BDH programme on first-born children school enrolment, indicating that having been exposed to the BDH cash transfers decreases the predicted probability of being enrolled in school among first-born children. However, interpretation of the coefficients in probit regressions is not as straightforward as in linear regression models. Specifically, a decrease in the probability of school enrolment attributed to a one-unit increase in a given regressor depends both on the starting value of the predictor under study and on the values of the other predictors. Holding all control dummies (explanatory variables) constant at zero, the predicted probabilities of being

enrolled in school for those first-born children affected and not affected by the BDH cash transfers are

$$Probability = F(constant + \ BDH\ coef. * BDH\ indicator)$$
$$F(0.5432 - 0.0313) = 0.6956 \ \ (affected\ by\ the\ BDH)$$
$$F(0.5432) = 0.7065\ (not\ affected\ by\ the\ BDH)$$

where F is the Cumulative Distribution Function (CDF) of the standard normal. Therefore, according to these results, first-born children affected by the BDH are about 1 percentage point less likely to be enrolled in school.

The results from including household's average educational attainment as an explanatory variable are reported in the second column. Here, the coefficient on the BDH indicator becomes insignificant, suggesting that *all* the negative effect on the probability of school enrolment was driven by the lower educational attainment of BDH beneficiary households, which is consistent with the previously reported findings about the effect of the BDH on educational attainment (see Table 17). In particular, if we hold all the other regressors apart from the BDH indicator constant at zero, then the predicted probability of first-born children enrolled in school will be of about 34.14 percent $[\mathrm{F}(-0.4084)]$ for both beneficiaries and non-beneficiaries of the BDH. Note that, when household's quality of floor material, the unemployment indicator and individual's age are progressively included in the rest of the specifications (columns 3, 4 and 5), the BDH coefficient remains statistically insignificant.

In the same table, I present estimates for the effect of the BDH programme on the probability of school attendance once the first-born child is enrolled using the same regression strategy. As shown in the second half of Table 19, these results are quite like those obtained in the previous section. In the most basic specification (column 1), the coefficient on the BDH indicator is again negative and strongly significant, showing that those first-born children affected by the BDH programme are about 2 percentage points

less likely to be attending school (holding all other explanatory variables constant at zero) $[F(0.4430 - 0.0328) - F(0.4430)]$.

As before, when the household's average educational attainment is included in the analysis (column 2), the estimated impact of the BDH on the probability of school attendance becomes economically and statistically insignificant even at the 10 percent level. Thus, indicating once more that the negative BDH effect is completely attributable to the lower educational level of the programme beneficiary families. Finally, in the rest of the specifications (columns 3, 4 and 5), the impact of the BDH remains insignificant. It seems therefore unlikely that there are any direct effects of the BDH cash transfers on school attendance of the first-born. Overall, these results suggest that the initially estimated impact of the BDH on school enrolment and attendance of the first-born is only due to differences in the other explanatory variables (between those affected and those not affected by the programme). However, it is important to mention that there is still a latent possibility that the effects of the BDH are reflected only in the educational outcomes of the younger siblings.

6.3.4 The Effects of the Bono de Desarrollo Humano on Unemployment[80]

In order to estimate the effects of the BDH on the probability of unemployment, I use the Registro Social data on employment status of all sampled individuals in both registers, including of course households without children. The sample includes individuals employed in different economic activities and those who are not working –but are seeking employment– at the time of the interview. This information allows me to construct a dummy variable for being unemployed, which is used as the dependent variable in the unemployment regressions to estimate the effects of the BDH social assistance

[80] The unemployed are defined in Ecuador as those adult people (gender neutral) able, available and willing to work, but who cannot find a job despite an active search for work.

programme on labour supply behaviour. Recall that I use probit regressions for the analysis when the outcome variable is dichotomous as in this case. The probit regression results for the effects of BDH cash transfers on unemployment status are presented in Table 20. As in previous analyses, initially the set of control dummies includes individual's province of residence, marriage status and ethnicity fixed-effects. Household's average educational attainment, quality of floor material and individuals' age are progressively included in the specifications as regressors.

In the first and most basic specification (column 1, Table 20), the estimated BDH coefficient is statistically significant (even at the 1 percent level) and negative, indicating that having been affected by these cash transfers in the past slightly decreases the predicted probability of being unemployed today. More specifically, holding all control dummies (explanatory variables) constant at zero, the predicted probabilities of being unemployed –for those individuals affected and not affected by the BDH cash transfers– are calculated as follows, drawing on the results presented in Table 20:

$$Probability = F(constant + \ BDH\ coef. * BDH\ indicator)$$
$$F(-2.4137 - 0.3721) = 0.0026 \ \ (affected\ by\ the\ BDH)$$
$$F(-2.4137) = 0.0078\ (not\ affected\ by\ the\ BDH)$$

where F is the cumulative distribution function of the standard normal. Therefore, according to these results, individuals affected by the BDH are 0.52 percentage points less likely to be unemployed $[F(-2.4137) - F(-2.4137 - 0.3721)]$.

The estimated impacts of the BDH programme are very similar when the household's average educational attainment is incorporated in column 2; and when the quality of floor material (column 3) and age (column 4) are also included as explanatory variables in the regressions. In the second specification, the coefficient on the BDH indicator is again negative and strongly significant, showing that those individuals affected by this type of cash transfers are about 0.3 percentage points less likely to be unemployed (holding all other explanatory variables constant at zero) $[F(-2.6197) -$

$F(-2.6197 - 0.3534)]$. This result, somewhat smaller in magnitude compared to the one in the most basic specification, suggests that some part of the negative effect on the probability of unemployment was due to differences in the educational level of the households. More specifically, due to a lower educational attainment of BDH beneficiary households (given the positive relationship found between household's educational attainment and the probability of unemployment), which is also consistent with the results reported in Table 17.

In the third specification, when household's quality of floor material is included in the analysis, the estimated effect of the BDH on unemployment is still economically and statistically significant. If we hold all the other regressors constant at zero, the negative impact of the BDH programme on the predicted probability of being unemployed will be of about 0.25 percentage points $[F(-2.6669) - F(-2.6669 - 0.3524)]$. Note that the BDH coefficient is slightly reduced in magnitude, indicating once more that part of the negative effect is attributable to the apparently lower income of households affected by this type of cash transfers (given the positive relationship found between household's quality of floor material and the probability of unemployment). These results are consistent with the regression estimates presented in Table 18.

Finally, in the full specification (column 4), the negative impact of the BDH cash transfers remains statistically significant at the 1 percent level. Once the age of individuals is controlled, those affected by the programme are about 0.52 percentage points less likely to be unemployed (holding all other explanatory variables constant at zero) $[F(-2.6197) - F(-2.6197 - 0.3534)]$. The slight increase in the magnitude of the coefficient suggests that part of the negative effect was being cancelled by age differences among individuals in the treatment and control groups. Given the negative relationship found between age and probability of unemployment, it seems that those individuals affected by the BDH are somewhat younger. Thus, the inclusion of age as a regressor guarantees that the negative impact of the BDH on unemployment is not being driven by the less labour experience (approximated by age) of cash transfers-affected individuals.

6.3.5 Lessons from the Natural Experimental Results

In general, the natural experimental results indicate that the BDH programme has had a small negative effect on welfare among those in poverty. Specifically, the negative impact is concentrated on two of the most important welfare components, such as the households' level of education and quality of floor material. On the other hand, I didn't find statistically significant effects of the BDH cash transfers on the probability of school attendance and school enrolment of the first-born child. Finally, the impact of the BDH programme on unemployment appears to be negative but small. The obtained results indicate that receiving the BDH cash transfers slightly reduces the probability of being unemployed. In statistical terms, the effect of the BDH programme on the probability of unemployment is around -0.5 and -0.3 percentage points on average for a given individual; and it remains practically unchanged as the number of controls increases in different regression specifications.

The estimated results imply that the efficiency levels that the BDH programme presents are not, in any way, what *should* be according to the theories of change described in the previous chapter (see page 186). When comparing the estimated effects with the expected results of the BDH, in terms of the well-being of people living in poverty, we can notice that the programme does not really promote investment in human capital at the household level or improve living conditions of families. Similarly, the BDH does not meet the objective of improving income levels and, therefore, ameliorating consumption patterns. As for the inter-temporal transmission of poverty, cash transfers (and their conditions) are not sufficient to promote school enrolment and attendance, as they theoretically should. Only in terms of encouraging good habits and practices, it could be said that the programme at least meets to some extent with the expected results, since it slightly discourages the probability of being unemployed. Therefore, it is evident that both the type of intervention and the dynamics of change of the BDH programme may not be sufficient to achieve the expected results or, perhaps, they are not being applied correctly. Unfortunately, the scope of this research does not allow me to determine the specific

aspects (resources/inputs, activities, mechanisms or channels of change) that should be corrected so that the intervention works correctly or at least more efficiently.

The overall findings of the proposed natural experiment largely discourage (at least in Ecuador) the implementation of traditional social assistance programmes to working-age families. Note that the estimated BDH effects on most outcome variables are insignificant or even negative, except for the impact on unemployment (which suggests that the BDH slightly reduces the probability of being unemployed). However, recall that although the BDH cash transfers are theoretically conditional on human capital investment, they do not impose work-related requirements. This means that even the optimistic unemployment regression results do not work in favour of conditional transfers that are exclusive to the *working* poor (as is the case in some developed countries). Therefore, the element of conditionality in social assistance, which has usually been thought of as indisputable, is seriously questioned in its effectiveness by these results. In other words, these findings are opposed to the idea of conditionalities as a necessary element to ensure the effectiveness of cash transfer programmes.

The paternalistic view of social assistance programmes –which clearly emphasizes the need to implement human capital- or work-related conditions for the delivery of cash transfers– is seriously challenged by the obtained results. On the other hand, alternative approaches such as the rational choice theory or even the welfare trap theory seem to be supported by the results of this experiment. It is possible that the negative effects of the BDH are due to the imposition of conditionalities that do not allow people to decide what is best for them in a free and rational way. This will become clearer when evaluating the results of the CDH programme, which does not impose any type of conditionality. Moreover, apart from the findings in terms of labour supply, these results strongly support the welfare trap hypothesis –since those individuals affected by the BDH present lower welfare conditions compared to those who did not receive cash transfers.

As a final remark, I reiterate the fact that although the treatment and control groups were virtually homogeneous in 2008, there were already some *small* differences between

groups, mainly in favour of the control group prior to the treatment implementation. These initial differences in the outcome variables are more visible in the case of the unemployment indicator (in this case favouring the treatment group). Thus, in terms of the unemployment indicator the control and treatment groups do not start from entirely similar situations. The BDH treatment group already had lower levels of unemployment in 2008. In other words, the groups are not homogeneous in terms of unemployment prior to the implementation of the BDH treatment. This circumstance could *at least partially* explain the estimated negative effects of the programme. This is a possibility that should be examined more closely using an alternative method that takes into account the data before treatment.

However, in defence of the natural experimental results obtained in this section, the difference between groups slightly increases in 2014 (see Tables 13 and 14). Therefore, a slight negative effect of the BDH programme may be expected, although perhaps smaller in size than the one estimated by the natural experimental method. Recall that the estimated effect does not consider the *initial* differences between groups (i.e. differences over time). In order to take into account these differences, alternative measurement mechanisms are required –such as is the difference-in-differences technique– which takes into account *both* the initial and final differences between the groups, allowing a more accurate estimate of the effects of the BDH. In the section below, I present difference-in-differences results using the same individuals for comparison reasons.

Table 16: Regression Estimates of the Effect of the BDH on the Welfare Index

(Standard Errors in Parentheses)

Independent variable	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
BDH Indicator	-2.1840*** (0.0220)	-1.4584*** (0.0209)	-1.1593*** (0.0183)	-1.1549*** (0.0183)	-1.1912*** (0.0181)
Household's Educational Attainment	---	2.7975*** (0.0079)	2.1759*** (0.0070)	2.1743*** (0.0070)	2.4617*** (0.0073)
Household's Floor Material	---	---	3.9376*** (0.0071)	3.9375*** (0.0071)	3.9616*** (0.0071)
Unemployment Indicator	---	---	---	0.5111*** (0.0767)	0.7061*** (0.0760)
Age	---	---	---	---	0.1123*** (0.0007)
Constant	23.3073*** (0.0666)	17.4001*** (0.0650)	2.9367*** (0.0628)	2.9366*** (0.0628)	-1.8771*** (0.0707)
Reported Province of Residence Dummies	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓
Number of Observations	997,437	997,437	997,437	997,437	997,437
R-squared	0.1762	0.2666	0.4366	0.4366	0.4479

Note: Estimates are from linear regression models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)

Table 17: Household's Educational Attainment Regressions (BDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
	Dependent Variable: Household's Average Educational Attainment							
BDH Indicator	-0.3577***	0.6992***	-0.3369***	0.7139***	-0.3322***	0.7172***	-0.3171***	0.7282***
	(0.0039)	(0.0027)	(0.0039)	(0.0028)	(0.0040)	(0.0028)	(0.0040)	(0.0029)
Household's Floor Material	---	---	0.2458***	1.2786***	0.2455***	1.2783***	0.2285***	1.2568***
			(0.0015)	(0.0020)	(0.0015)	(0.0020)	(0.0015)	(0.0019)
Unemployment Indicator	---	---	---	---	0.5156***	1.6746***	0.4094***	1.5060***
					(0.0168)	(0.0281)	(0.0168)	(0.0254)
Age	---	---	---	---	---	---	-0.0492***	0.9519***
							(0.0001)	(0.0001)
Number of Observations	997,437	997,437	997,437	997,437	997,437	997,437	997,437	997,437
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 18: Household's Quality of Floor Material Regressions (BDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
Dependent Variable: Household's Quality of Floor Material								
BDH Indicator	-0.2017***	0.8172***	-0.1380***	0.8710***	-0.1372***	0.8717***	-0.1361***	0.8726***
	(0.0040)	(0.0033)	(0.0041)	(0.0035)	(0.0041)	(0.0035)	(0.0041)	(0.0035)
Household's Educational Attainment	---	---	0.2667***	1.3056***	0.2664***	1.3052***	0.2526***	1.2873***
			(0.0016)	(0.0020)	(0.0016)	(0.0021)	(0.0016)	(0.0021)
Unemployment Indicator	---	---	---	---	0.1013***	1.1066***	0.0920***	1.0963***
					(0.0175)	(0.0193)	(0.0175)	(0.0192)
Age	---	---	---	---	---	---	-0.0053***	0.9947***
							(0.0001)	(0.0001)
Number of Observations	997,437	997,437	997,437	997,437	997,437	997,437	997,437	997,437
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 19: Regression Estimates of the Effects of the BDH on School Enrolment and School Attendance of the First-born Child

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)	Probit Regression (5)
Dependent Variable: First-born Child Enrolled in School Dummy					
BDH Indicator	-0.0313*** (0.0084)	0.0017 (0.0087)	0.0056 (0.0087)	0.0051 (0.0087)	0.0031 (0.0087)
Household's Educational Attainment	---	0.4219*** (0.0046)	0.4150*** (0.0046)	0.4151*** (0.0046)	0.4124*** (0.0047)
Household's Floor Material	---	---	0.0426*** (0.0033)	0.0426*** (0.0033)	0.0419*** (0.0033)
Unemployment Indicator	---	---	---	-0.0636* (0.0405)	-0.0658* (0.0405)
Age	---	---	---	---	-0.002*** (0.0005)
Constant	0.5432*** (0.0226)	-0.4084*** (0.0255)	-0.5637*** (0.0283)	-0.5636*** (0.0283)	-0.442*** (0.0381)
Number of Observations	136,247	136,247	136,247	136,247	136,247
Dependent Variable: First-born Child Attending School Dummy					
BDH Indicator	-0.0328*** (0.0082)	-0.0016 (0.0084)	0.0032 (0.0084)	0.0027 (0.0084)	0.0003 (0.0085)
Household's Educational Attainment	---	0.3943*** (0.0044)	0.3859*** (0.0045)	0.3860*** (0.0045)	0.3829*** (0.0045)
Household's Floor Material	---	---	0.0522*** (0.0032)	0.0522*** (0.0032)	0.0514*** (0.0032)
Unemployment Indicator	---	---	---	-0.0547* (0.0394)	-0.0573* (0.0394)
Age	---	---	---	---	-0.002*** (0.0005)
Constant	0.4430*** (0.0220)	-0.4543*** (0.0247)	-0.6449*** (0.0275)	-0.6448*** (0.0275)	-0.498*** (0.0369)
Number of Observations	136,247	136,247	136,247	136,247	136,247
Province of Residence Dummies	✓	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.

*** Significant at the 1 percent level (P value<0.01)

* Significant at the 10 percent level (P value<=0.1)

Table 20: Unemployment Regressions (BDH)

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)
Dependent Variable: Dummy for Being Unemployed				
BDH Indicator	-0.3721*** (0.0090)	-0.3534*** (0.0091)	-0.352*** (0.0091)	-0.347*** (0.0091)
Household's Educational Attainment	---	0.0903*** (0.0026)	0.0884*** (0.0027)	0.0748*** (0.0028)
Household's Floor Material	---	---	0.0123*** (0.0031)	0.0111*** (0.0031)
Age	---	---	---	-0.006*** (0.0003)
Constant	-2.4137*** (0.0261)	-2.6197*** (0.0270)	-2.666*** (0.0296)	-2.395*** (0.0330)
Number of Observations	997,437	997,437	997,437	997,437
Province of Residence Dummies	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)

6.4 Difference-in-Difference Results

Although the randomization assumption (which was essential for the previous natural experiment) can be somewhat relaxed when using this method, there are other important assumptions that must be taken into account for its implementation and correct interpretation of the results. In other words, even if the individuals in the sample have not been assigned to the treatment and control groups by the implementers of the BDH programme, using a formal randomized mechanism (like a randomized control trial), this analysis can be carried out provided that certain conditions are met (Munoz, 2010).

As a general rule, several assumptions have to be made in order to allow for any consistent estimation of the causal effects. In this case, I will list the most important ones and those that we should keep in mind when analysing the results. First of all, the central assumption of the difference in-differences approach is that the underlying trends in the outcome variables (such as the welfare index, educational attainment or unemployment) would be the same for all individuals in the absence of the treatment, which is never possible to probe formally and constitutes a very strong and admittedly questionable assumption (Stuart et *al*., 2014). However, if the treatment effect survives even after controlling for individual- and group-specific characteristics, the identification strategy remains to be valid.

A common way of checking if this assumption seems at least plausible is to see if the trends in the outcome variables were parallel before the BDH intervention, which was already done when the pre-treatment characteristics of the sample were analysed in Chapter 5 (see page 191). Note that it isn't necessary to have random assignment of individuals for this assumption to hold, but it is much more likely to fail if the assignment was based on some particular characteristics of the groups.

Moreover, in the case of the present thesis, the validity of the identification strategy hinges as well on the assumption that interaction of group and time effects do not affect the outcome variables directly (only through the BDH effects). This assumption is

clearly more likely to be satisfied when, before the analysis, we restrict the sample to individuals with similar basic characteristics. Therefore, as explained in the sample selection process (see page 189), in order to estimate the regressions, I first restrict the evaluation sample to those working-age individuals without disabilities who, being in a situation of poverty, are potential beneficiaries of the BDH programme.

Finally, it is important to note that, specifically in the case of the BDH evaluation, the regressor of interest ($TIME2014 * BDH$) –conditional on year and group– is considered *as good as* randomly assigned. In other words, it is assumed that individuals cannot choose only by themselves to be affected or not by the programme. A possible concern would be if the composition of the treatment and control groups changes as a result of the BDH cash transfers implementation (e.g. the existence of selective migration). More specifically, there might be a problem if individuals could decide for themselves if they are going to be programme beneficiaries in response to the government officials' decision to introduce the BDH for those in poverty.

However, as explained in depth in Chapter 2 (see page 70), it is the public institution in charge of the programme that exclusively decides the recipients and potential recipients of the BDH monetary transfers based on the poverty condition of the households and the budgetary restrictions of each administration. In addition, it is difficult to think that households will voluntarily reduce their welfare situation in order to have more chances of being chosen, or even more unlikely that individuals will reject monetary transfers once they have been assigned. Therefore, potential self-selection issues –like the mobility among groups– are not likely to occur invalidating the results of the natural experiment. In other words, it is unlikely that a problem of selection bias will arise. Anyway, an important advantage of the diff-in-diff strategy is that it would partly overcome this still potential problem in case it is happening.

6.4.1 The Diff-in-Diff Effects of the Bono de Desarrollo Humano on the Welfare Index

The difference-in-differences estimates of the impact of the BDH cash transfers on the SELBEN welfare index are presented in Table 21. As in the natural experimental method, all the regressions in this table control for province of residence, marriage status, and self-defined ethnicity fixed-effects. The results are presented following a structure similar to that of the previous method, that is, the control variables are progressively included in the different specifications. In the first and most basic specification, the household's educational attainment, the quality of floor material the unemployment indicator and individual's age are not taken into account.

The first column indicates that with the mere inclusion of control dummies, there is a negative effect of the BDH programme on welfare. However, this impact is reduced by almost 50 percent compared to that estimated by the natural experimental method. Specifically, the results in column (1) show that the BDH cash transfers decrease the SELBEN welfare index by about 1.23 points, and that all the diff-in-diff estimates in the specification are highly statistically significant. Besides, the coefficient on the BDH indicator is relatively precisely estimated. It is possible to claim this because, although not included in Table 21, the 95 percent confidence interval for the effect of the BDH programme on the welfare index ranges from -1.28 to -1.18.

Before analysing the rest of the coefficients in the specifications, it is necessary to make some important general remarks. First, note that the number of observations has practically doubled (i.e. 1,989,708 observations), as was expected, since I have information at two different points of time. Moreover, the 2014-indicator and the treatment group indicator were included as independent explanatory variables in the regressions. It is important to include time and group indicators as separate control variables, because they are likely to influence the outcome variables directly and independently of whether an individual was affected by the BDH cash transfers. In other

words, I need to account for group specific characteristics and trends in time that could be affecting the estimated results. Finally, recall that the BDH indicator is constructed based on the interaction of an observation's time and group indicators, under the assumption that the only way through which the interaction term affects the outcome variables is through the BDH effects.

The estimated 2014-indicator coefficient is positive and statistically significant, indicating that if the observation belongs to the post-treatment period, the welfare index increases drastically for all individuals. That is, regardless of whether an individual is affected by BDH cash transfers, the trend over time is that his level of welfare improves by about 10 index points due to a combination of exogenous factors, which include societal, familial, economical, political and other similar conditions. On the other hand, the impact of the treatment group indicator is negative and statistically significant, although small in magnitude. The estimated coefficient indicates that if the observation belongs to an individual among the treatment group, the welfare index slightly decreases by about 0.97 points, independently of whether the observation belongs to the post- or pre-treatment period.

Therefore, there might be some intrinsic characteristics of the individuals that belong to the treatment group that negatively affect their level of well-being. These results suggest that both trends over time and group specific characteristics affect the results, and therefore, it is necessary to take them into account in order to obtain a more accurate estimator of the effect of the BDH cash transfers. Note that the reduction of the BDH coefficient is practically of 1 index point if we compare it with the one estimated by the previous method. This could be explained to a large extent by the inclusion of group-specific characteristics in the diff-in-diff method, since the effect of the treatment group indicator is also negative and of a similar magnitude to the mentioned decrease in the BDH coefficient.

The results obtained in the first specification do not vary much when the household's average educational attainment is included in column (2). The BDH estimate

is still negative and highly statistically significant, showing that the impact of the BDH is not cancelled by the estimated positive effect that each completed educational degree has on welfare (2.39 index points per additional degree). Nevertheless, the BDH effect is reduced to -0.93 index points, indicating that a small part of the negative impact on welfare can be linked to the direct negative effect that the BDH cash transfers should have on the household's average educational attainment, which will be confirmed in the next section of this chapter (see Table 22).

Similarly, the treatment group indicator coefficient remains negative and significant, suggesting that individuals in the treatment group in general present slightly lower levels of well-being (-0.64 index points), regardless of whether they are beneficiaries of the BDH cash transfers. Note also that this coefficient has been reduced by the inclusion of average educational attainment in the regression, indicating that part of the welfare differences among groups could be explained specifically by differences in the level of instruction. In this case, the results show that individuals in the treatment group have a slightly lower level of education (as part of the group-specific characteristics), which in turn affects negatively their level of well-being. Moreover, the estimated impact of the 2014-indicator on well-being is practically the same as in the previous specification (i.e. 10 index points). In other words, the presence of the household's average educational attainment does not change the fact that the welfare index increases considerably in the post-treatment period for all the individuals.

In column (3), when the household's quality of floor material (a common proxy of the income level) is included as an explanatory variable of the level of well-being, the BDH coefficient is once again negative and highly statistically significant. In fact, all the coefficients in this specification remain practically unchanged if not for a slight reduction in their magnitude. More specifically, the results show that the BDH cash transfers decrease the SELBEN welfare index by about 0.85 points (that is 0.08 index points less than in the previous specification). This small reduction in the BDH coefficient, together with the currently estimated positive impact of quality of floor material on well-being (3.15 index points), imply that there might be some direct negative effect of the BDH

programme on the household's proxy of income level. This implication is going to be analysed in the next section, but these results already give us an idea of what we can find (see Table 23).

Additionally, once the quality of floor material is included in the regression, the estimated coefficients of the treatment group indicator and the 2014-indicator do not vary too much. The results suggest that the trend over time is still that the level of welfare for all individuals improves by about 8.55 index points between 2008 and 2014, while the specific characteristics of the individuals in the treatment group affect negatively their level of well-being by about 0.46 index points, independently of whether the observation belongs to the post- or pre-treatment period (i.e. regardless of the BDH cash transfers effect). However, the magnitude of both indicators' coefficients is slightly reduced by the inclusion of the household's quality of floor material, indicating that some part of the welfare differences among groups and years could be explained specifically by differences in the income level.

Finally, with regards to the BDH estimate, there are hardly any changes to coefficients when I gradually include controls for individuals' unemployment situation (column 4) and age (column 5). In the fourth specification, the BDH coefficient is again negative, highly statistically significant and its magnitude is of about 0.85 SELBEN index points (less than one point). The same can be said about the fifth specification when the age of individuals is included in the diff-in-diff regression. As in the previous specifications, it can be stated that the coefficients in both regressions are relatively precisely estimated, given that with a confidence level of 95 percent the BDH effects lie between -0.89 and -0.81 points. Note that, given that the BDH coefficient is not altered by the inclusion of the unemployment indicator, the results suggest that the effects of the cash transfers on welfare do not channel through their independent effect on the labour situation of individuals. In fact, it is most likely that this time, contrary to the natural experimental findings, no direct effect at all (or a very small one) will be found of the BDH programme on unemployment status when analysed in depth later (see Table 25).

Also, the estimated impacts in columns (4) and (5) of the trends over time (i.e. 2014 indicator) and group-specific characteristics (i.e. treatment group indicator) on the individuals' welfare index are very similar to those found above. Specifically, the treatment group indicator coefficient is still negative and highly statistically significant, showing that individuals labelled as the treatment group in general present slightly lower levels of well-being (-0.46 index points). The small reduction of the coefficient, compared with those found in the previous specifications, implies that one of the specific characteristics of the groups –which affect their level of well-being– is related to the probability of unemployment. In this case, the results indicate that the treatment group presents lower levels of unemployment (regardless of the BDH cash transfers), and this fact could partially explain their estimated lower level of well-being.

These latest findings are supported by the conducted analysis of the characteristics of the groups prior to the BDH treatment intervention (see Table 13) and they allow us to practically rule out the idea –suggested by the natural experiment– that BDH cash transfers beneficiaries are less likely to be unemployed. Granting practically all the credit of the differences in the level of unemployment to the intrinsic characteristics of the groups (and no longer to the BDH programme), which were of course omitted by the natural experimental approach.[81] On the other hand, the effect of the 2014-indicator on well-being is almost the same as in the third specification (i.e. increase in SELBEN by about 8.55 index points). Thus, the presence of the unemployment indicator and individuals' age does not change the fact that the welfare index increases considerably in the post-treatment period induced by external factors. Although, it is necessary to recognize that once age is controlled for in the last specification, there is a slightly reduction on the estimated impact of trends over time on welfare.

Overall, the diff-in-diff regression results in Table 21 confirm that BDH conditional cash transfers had a slight but negative effect on the welfare level of those in

[81] The diff-in-diff effects of the BDH programme on unemployment status will be analysed in more detail in a later section of this chapter, where the probability of unemployment is taken as the outcome variable.

poverty. Although the effects are indeed small in magnitude, they represent a relatively significant decrease in the level of well-being considering that the average welfare of the sample in 2008 (i.e. before the intervention) was only 21 index points, and that the difference between the lowest percentile (i.e. the poorest 1 percent) and highest percentile (i.e. the 1 percent less poor among the poor) was about 28 index points.

As more control variables are added to the diff-in-diff regressions, the magnitude of the BDH effect on the welfare index is gradually reduced from -1.23 points until it stabilizes (in the third specification) at about -0.85 points. Moreover, as was also the case in the natural experimental design, the independent effects of household's educational attainment, quality of floor material (as an income proxy), unemployment status, and individual's age (as a labour experience proxy) on welfare are positive and statistically significant.

Note that the F values of the model are highly statistically significant as well. This denotes that the explanatory variables used in the specifications have a significant explanatory power as a whole. Nevertheless, the diff-in-diff model once again treats these explanatory variables as if they were continuous, despite the fact that some of them are ordinal discrete variables. As previously explained, the advantage of this technique is that it incorporates into the model the fact that each variables' categories are ordered, but at the cost of assuming that equal changes in X mean equal changes in Y.

For instance, in the specification with all controls included (Column 5), the effect of quality of floor material is interpreted as the difference in the predicted value of the welfare index for each-one unit difference in the predictor, if all other independent variables remain constant. In this case, note that a one unit difference in the predictor represents switching from one floor material category to the next higher or lower. Therefore, the average difference in welfare between floor material categories is 3.16 index points. Similarly, the increase of a category in the average level of education of the household has an effect of 2.17 points on the SELBEN welfare index, while the effect of each additional year of age is only 0.07 points.

Finally, in my diff-in-diff regression analysis, I found R-squared values ranging from 34 percent in the first specification to 52 percent in the full specification (see last row of Table 21), which are considerably higher than those estimated in the natural experimental model. Thus, if the primary objective would be to produce predictions of the SELBEN welfare index that are reasonably precise, then the estimated R-squared values would be no longer an important concern. Specifically, the coefficient of determination obtained in the last specification indicate that the model has little "prediction error" and, thus, that making projections of the welfare index would be possible –given the explanatory variables that I have included in this regression. In fact, note that in this case less than half of the variation in the outcome variable should be explained by unobserved factors.

However, as stated above, making predictions is not the objective of the diff-in-diff model applied in this thesis. Actually, its main purpose is to determine which of the selected predictors are statistically significant and how changes in the explanatory variables relate to changes in the welfare index. Therefore, in this case, the R-squared values are again almost totally irrelevant, since they do not need to be above any particular value to validate the obtained results. In other words, once the regression model is correctly specified, the R-squared does not affect the way we interpret the estimated coefficients. Recall that the interpretation of the coefficients and their statistical significance is the same regardless of whether the coefficient of determination value is 10 percent or 95 per cent.

6.4.2 The Diff-in-Diff Effects of the Bono de Desarrollo Humano on Two Different Welfare Components

The difference-in-differences estimates of the impact of the BDH cash transfers on the households' average educational attainment and housing conditions are presented in Tables 22 and 23. The key results are that the BDH cash transfers cause a small reduction

in the average educational attainment of the household and in the quality of the floor material. As in all the regressions estimated in this thesis (irrespective of the statistical method), the specifications in these tables control for province of residence, marriage status, and self-defined ethnicity fixed-effects. Once again, the structure used for presenting the results is based on the progressive inclusion of more control variables, which include the unemployment indicator and individual's age.

Moreover, recall that the difference-in-differences method requires the inclusion of the 2014-indicator and the treatment group indicator as independent explanatory variables in the regressions in order to account for group specific characteristics and trends in time that could be affecting the estimated results. The BDH indicator is constructed based on the interaction of these two variables (i.e. an observation's year and group indicators).

As previously explained (see page 176), in this case the dependent variables (i.e. educational attainment and quality of floor material) are ordinal categorical (or ordinal discrete) measures of human capital and income. Households' educational attainment is a categorical outcome variable describing the self-reported educational degree of the families as a rounded average of its members ranking it into nine categories. Similarly, the floor material outcome variable specifically describes its self-reported quality using eight different categories. Both rankings are classified from the lowest to the highest level.

It has also been stated before that the theory of statistical modelling requires ordered logistic regression models –also known as ordered logit or proportional odds models– to analyse this type of dependent variables given their level of measurement. Recall that these models predict the likelihood of an individual appearing in each successively higher category of the outcome variable. Tables 22 and 23 summarize, respectively, the ordered logistic diff-in-diff regression results for the effects of the BDH cash transfers on the households' level of education and on the quality of floor material. In each one of the tables, the first half of each column (or specification) interprets the

coefficients in terms of ordered log-odds (logits) and the second half interprets the coefficients in terms of proportional odds.

The results presented in Table 22 confirm that the average educational attainment of households is one of the components of welfare on which the BDH has a negative and statistically significant effect. In the first and most basic specification (column 1), the ordered log-odds (logit) coefficient on the BDH indicator is -0.1754. Therefore, the ordered log-odds for BDH-affected individuals of being in a higher education category are 0.1754 less than individuals not affected, when the other variables are held constant. In terms of simple probability, this means that if an individual receives BDH transfers the probability (p) that his household is at a higher category of education is 0.4563. That is to say, it is slightly more likely for the household of a given individual to be at a higher education category if the individual is not affected by the BDH (p = 0.5437) (see Appendix D).

The second half of column (1) displays the coefficients in terms of proportional odds ratios, which allow for the comparison of individuals in groups above a given level (k) with individuals in groups below the same level. In this case, 0.8390 is the estimated proportional odds ratio of comparing individuals affected by the BDH to those not affected in terms of educational attainment of the household. This coefficient indicates that for BDH-affected individuals the odds of being in a higher category than, for example, secondary education (k), versus the rest of the categories combined, are 0.8390 times (thus, lower) the odds for those not-affected individuals, given that the other variables are held constant in the model. Note that the ordered logistic estimates of the first column can be related by the following formula:

$$0.8390 \text{ (proportional odds ratio)} = \frac{0.4563\ (p \text{ of being in a higher education category for BDH affected individuals})}{0.5437\ (p \text{ of being in a higher education category for those not affected by the BDH})}$$

It is also worth noting that in column (1) the estimated ordered log-odds (logit) coefficient on the 2014-indicator is positive and significant (i.e. 0.2310), indicating that if an individual's observation belongs to the post-treatment period, the probability (p) that his household is at a higher category of education is 0.5575. Therefore, it is more likely for the household of a given individual to be at a higher education category if the observation belongs to the post-treatment period. In fact, the proportional odds ratio shows that, for post-treatment observations, the odds of being in a higher education category are 1.2598 times (therefore, greater) the odds for pre-treatment observations (p = 0.4425). These results may seem intuitive, but they imply that on average households in poverty improved their average levels of education over time, regardless of whether the individual is affected by the BDH cash transfers.

Moreover, the impact of the treatment group indicator is negative and statistically significant. The estimated ordered log-odds coefficient (i.e. -0.2241) indicates that if the observation belongs to an individual among the treatment group, the probability that his household is in a higher category of education (p = 0.4442) is lower than the probability if the individual belongs to the control group (p = 0.5558). Thus, the proportional odds ratio (presented in the second half of the column) is 0.7992 (POR= 0.4442/0.5558), which means that the odds of being in a higher education category for individuals in the treatment group are 0.79 times the odds for individuals in control group. This is independently of whether the observation belongs to the post- or pre-treatment period and, of course, irrespective of receiving BDH cash transfers. Therefore, there might be intrinsic characteristics of the individuals in the treatment group that negatively affect the average educational attainment of their households.

The ordered log-odds coefficients and the proportional odds ratios estimated in the first specification do not vary much when more control variables are included in columns (2), (3) and (4). More specifically, the impacts of the BDH programme, the 2014-indicator and the treatment group indicator on educational attainment are practically the same when floor material, unemployment and age are progressively included in the regressions. The small but existing reductions in the magnitude of the BDH log-odds coefficient indicate

that a very small part of the effect of the programme was due to the housing conditions, the employment status and the age of the individuals.

Additionally, the diff-in-diff findings also suggest a positive and significant effect of the quality of floor material (income proxy) and unemployment on educational attainment, indicating that better housing conditions and being unemployed have positive independent impacts on the average level of education of the households. As previously mentioned, one possible explanation could be that higher levels of income and more time available improve the probability of achieving a higher educational degree. On the other hand, individuals' age has a very small but negative effect on educational attainment.

Overall, these results are consistent with those estimated using the natural experimental method. The statistical significance and direction of the explanatory variables' coefficients are very similar in both methods. For instance, they indicate that the BDH has a significant negative effect on the average level of education of poor families. Although it is important to note that the magnitude of the BDH log-odds coefficient is considerably smaller using the diff-in-diff approach. Once again (like in the case of the welfare index), this suggests that both trends over time and group specific characteristics affect the results, and therefore, it is necessary to take them into account using a diff-in-diff technique in order to obtain a more accurate estimator of the effect of the BDH cash transfers.

It is now the turn to analyse the effects of the BDH programme on another important well-being component, such as the housing conditions. More specifically, the quality of the floor material is taken as the dependent variable mostly because it is a good and commonly used proxy for the income level of the families. The results reported in Table 23 show that the household's quality of floor material is another component of welfare on which the BDH has a negative and statistically significant impact. However, the magnitude of the order logit coefficient is very small in absolute terms and close to

the log-odds scale centre of 0.0, suggesting that the BDH effect will also be small. This will be reflected clearly when interpreted in terms of probabilities.[82]

In the first specification (column 1), the estimated ordered logit coefficient on the BDH indicator is -0.0775 log-odds units. Thus, the straightforward interpretation would be that the ordered log-odds for BDH-affected individuals of being in a higher floor material category are 0.0775 units less than individuals not affected, when the other variables are held constant. In terms of probabilities (which might be easier to interpret), this means that if an individual receives BDH transfers, the probability (p) that his household is at a higher category of floor material is 0.4806 probability units. That is to say, it is slightly less likely for the household of a given individual to be at a higher floor material category if the individual is affected by the BDH cash transfers. Note that if the probability were 0.5, this would mean that both (affected and unaffected) have the same probability of being in a higher category.

The same BDH logit coefficient, but in terms of proportional odds ratio (presented in the second half of column 1) is 0.9253. Recall that the proportional odds ratio is obtained by dividing the probability of being in a higher floor material category for BDH-affected individuals by the probability for those not affected. Therefore, this coefficient indicates that the probability for BDH-affected individuals will be slightly smaller and, more specifically, will be equal to 0.9253 times the probability for those not affected individuals. As you can see, both ordered logit and proportional odds models suggest small but significant negative effects of the BDH cash transfers on the quality of floor material.

Besides, the estimated ordered logit coefficient on the 2014-indicator is positive and statistically significant, as was the case when analysing the impact of this indicator on the welfare index and the educational attainment of the households. The magnitude of the coefficient is very large (i.e. 0.9146 log-odds units), suggesting that if an individual's

[82] Notice that a log-odds of 0 responds to a probability of 0.5 and that as probabilities increase, the log-odds also increase.

observation belongs to the post-treatment period, the probability (p) that his household is at a higher category of floor material is 0.7139. An alternative way to see it is through the proportional odds ratio, which shows that the probability for post-treatment observations of being in a higher floor material category are 2.4959 times the probability for pre-treatment observations. These results clearly indicate that poor households improved their housing conditions over time, regardless of whether they were affected by the BDH cash transfers.

Similarly, the effect of the treatment-group indicator is negative and statistically significant, as was also the case when analysing the impact of this indicator on the welfare index and the educational attainment of the households. Specifically, the estimated ordered log-odds coefficient (i.e. -0.1344 log-odds units) indicates that if the observation belongs to an individual among the treatment group, the probability that his household is at a higher category of floor material is 0.4664 probability units, which therefore is slightly lower than the probability for individuals in the control group (i.e. 0.5336 probability units). In terms of the proportional odds ratio, the odds of being in a higher floor material category for individuals in the treatment group are 0.8741 times the odds for individuals in control group. Although the magnitude of the ordered logit coefficient is small, these results confirm that there are intrinsic characteristics of the individuals in the treatment group that negatively affect their level of well-being through more than one of its components.

The progressive inclusion of more control variables (all of them statistically significant) in columns (2), (3) and (4) hardly reduces the magnitude of the ordered log-odds coefficients, but it does not alter their interpretation at all. For instance, in the full specification (column 4), the estimated ordered logit coefficient on the BDH indicator is -0.0493 log-odds units (i.e. about 0.02 less negative than in the first specification). However, in terms of probabilities, there are no significant changes. This coefficient still means that if an individual receives BDH transfers the probability that his household is at a higher category of floor material is of about 0.48 probability units. The same applies to the rest of the estimated ordered log-odds coefficients and proportional odds ratios.

In general, the diff-in-diff results on quality of floor material are very consistent with those estimated using the natural experimental method. More specifically, the statistical significance and the direction of the estimated coefficients are the same with both methods. Being the small but negative effect of the BDH programme the most important finding. Note that although the magnitude of the BDH ordered log-odds coefficient is considerably smaller in the diff-in-diff approach (-0.0493 log-odds units) than in the natural experiment (-0.1380 log-odds units) –suggesting that trends over time and group specific characteristics should be taken into account–, the interpretation of these coefficients in terms of probability shows that there is not a significant difference between both results. In fact, the probability (of being in a higher floor material category for BDH-affected individuals) that corresponds to each of the ordered log-odds coefficients is 48 and 46 percent respectively.

6.4.3 The Diff-in-Diff Effects of the Bono de Desarrollo Humano on Human Capital of the First-born Children

In light of the previous results, it is still possible that the BDH beneficiary households are complying with the programme's attached conditions related to the education of the children, although this is not yet reflected in the average educational achievement of the households. In order to test this hypothesis, using this time a quasi-experimental method, I estimate difference-in-differences regressions of the impact of the BDH cash transfers on school enrolment and school attendance of the first-born children.

The obtained results are summarized in Table 24. All the specifications in the table control for individual's province of residence, marriage status and ethnicity fixed-effects. Moreover, household's average educational attainment, quality of the floor material, unemployment status and age are progressively included in the specifications as other explanatory variables. As explained in the natural experimental approach (see page 219), I first constructed indicators (i.e. dummy variables) of school enrolment and school

attendance for the first-born children. Due to the binary discrete nature of these dependent variables, I use probit regression models for the analysis. Recall that these models calculate the predicted probability of being enrolled and attending school on the basis of the regressors used. They do so using the Cumulative Distribution Function (CDF) of the standard normal.

The first half of Table 24 presents the probit diff-in-diff regression results for the effects on the likelihood that the first-born children are *enrolled* in school. In column (1), the BDH coefficient is statistically insignificant, suggesting that having been exposed to the conditional cash transfers does *not* affect the predicted probability of school enrolment of the first-born. Thus, in spite of the conditionalities to which the beneficiaries of the programme are tied, it is equally probable that both they and the non-beneficiaries are enrolling their children in school (at least their oldest child). The actual value of this probability will of course depend on the initial values of *other* predictors that are statistically significant.

In the same column, the estimated coefficient on the 2014-indicator is positive and significant (i.e. 0.0751), indicating that if an individual's observation belongs to the post-treatment period, then the expected probability of his first-born being enrolled in school slightly increases. In other words, it is a bit more likely for the first-born of a given individual to be enrolled in school during the post-treatment period. However, recall that the interpretation of the coefficients in probit regressions is not as straightforward as in linear regression models. Using the CDF of the standard normal and holding the values of all the other predictors constant at zero, the predicted probabilities of the first-born enrolling in school for observations in the pre- and post-treatment periods are as follows:

$$Probability = F(constant + \ 2014\ coef.* 2014\ indicator)$$
$$F(0.4426 + \ 0.0751) = 0.6976 \ \ (post\text{–}\ treatment\ period)$$
$$F(0.4426) = 0.6709\ (pre\text{–}\ treatment\ period)$$

where F is the CDF of the standard normal. According to these results, first-born children in the post-treatment period are about 2 percentage points more likely to be enrolled in school, holding everything else constant. Therefore, the trend over time is for households in a situation of poverty to somewhat improve their behaviour towards their children's human capital, regardless of the BDH cash transfers.[83]

On the other hand, the impact of the treatment group indicator is negative and statistically significant. Although the magnitude of the effect is small, the estimated coefficient (-0.0350) suggests that if the observation belongs to an individual in the treatment group, then the expected probability of his first-born enrolling in school is slightly lower than the probability for individuals in the control group. In particular, if we hold all the other regressors constant at zero, then the predicted probability of the first-born enrolling in school will be of about 65.82 percent $[F(0.4076)]$ for individuals in the treatment group and 67.09 percent $[F(0.4426)]$ for individuals in the control group. Thus, the negative effect of the treatment group indicator on the probability of school enrolment is of 2 percentage points. Of course, this impact is independent of whether individuals receive BDH cash transfers, and reflects the presence of intrinsic characteristics in the treatment group that negatively affect the likelihood of school enrolment.

This last statement is confirmed in the second column when household's average educational attainment is included as an explanatory variable. The coefficient on the treatment group indicator becomes statistically insignificant indicating that all the negative effect was driven by lower levels of education in the treatment group. Once this unobserved characteristic (in the first specification) is controlled for (in the second specification), the impact of the treatment group indicator disappears. Something similar happens with the 2014-indicator, whose effect was positive and significant until the educational attainment of households is included. In the second specification, this effect

[83] However, the effect of the 2014-indicator becomes negative and significant once educational attainment is controlled in the next specification. Thus, all the positive effect was due to the evidently higher levels of education in the post-treatment period and the actual trend over time is for households to worsen their probability of enrolling the first-born in school.

becomes negative indicating that all the positive effect was driven by higher levels of education in the post-treatment period.

The coefficients in the rest of the specifications remain practically unchanged. In fact, the BDH indicator coefficient is still negative but insignificant in column (2) and when more control variables are progressively included in the other specifications. Although, it is worth noting that in the last specification a very small effect of the BDH programme becomes marginally significant (at the 10 percent level), suggesting that, if anything, being exposed to the BDH cash transfers could very slightly reduce the predicted probability of the first-born being enrolled in school (i.e. about 0.5 percentage points).

In the second half of Table 24, I present (probit) diff-in-diff estimates for the effects of the BDH programme and the other regressors on the probability that the first-born children are *attending* school. The overall results are very similar to those in the first half of the table for a similar outcome variable. More specifically, the coefficients on the BDH indicator are again small, negative and statistically insignificant in most of the specifications, with the exception of the full regression (column 5) where the BDH indicator is just marginally significant. Thus, these results suggest that those first-born children affected by the BDH programme are not more or less likely to be attending school despite of the cash transfers. If anything, they are about 0.5 percentage points less likely to be attending school (holding all the other explanatory variables in columns 5 constant at zero) $[F(-0.3402) - F(-0.3402 - 0.0171)]$.

Moreover, the coefficients on the 2014-indicator are again negative and significant once educational attainment is included in column (2) and also in the following specifications, confirming that the trend over time is for households to worsen their behaviour related to investment in human capital of children, specifically, to reduce their probability of sending the eldest son to school in the post-treatment period. Finally, the coefficients on the treatment group indicator are again statistically insignificant from the second specification. The inclusion of educational attainment (column 2) cancels the initially estimated negative effect of the treatment group (column 1). Therefore, there are

no intrinsic characteristics of the treatment group (beyond education levels) that are affecting the likelihood of sending children to school.

Like the regression results obtained in the previous natural experiment, the diff-in-diff results show that the BDH has no impact on the probability of school enrolment and attendance of the first-born children. If anything, there is a very small negative effect on both outcome variables (i.e. around 0.5 percentage points), but this effect is only marginally statistically significant. Therefore, the results obtained by both methods are consistent with each other and they at least question the efficacy of conditionalities related to the education of the children. According to these results, and contrary to what might be expected, being a beneficiary of the BDH conditional cash transfers does not increase the probability of enrolling or sending the first-born children to school.[84]

In the case of this CCT programme, these results could be explained by inefficient monitoring and control of conditionalities, but it is important to note that there is also the possibility that this type of social assistance programme is not the most appropriate to modify the behaviour of those in poverty (at least in the Ecuadorian case). A greater investment in the human capital of the children could, in fact, be one of the consequences of having reached an adequate level of well-being by families, instead of a necessary condition to reach that level of well-being. In any case, attaching conditions to payments does not seem to be the most effective way to help families to overcome poverty in the medium or long term.

[84] However, there is still a latent possibility that the effects of the BDH are reflected in the educational outcomes of the younger siblings.

6.4.4 The Diff-in-Diff Effects of the Bono de Desarrollo Humano on Unemployment

The diff-in-diff regression results for the effects of the BDH cash transfers on the probability of unemployment are presented in Table 25. Key findings are that BDH cash transfers cause a statistically significant reduction in the probability of unemployment, although the magnitude of the effect is not considerable. All specifications include a set of control dummies (i.e. individual's province of residence, marriage status and ethnicity fixed-effects). More control variables are progressively included as in the previous natural experimental models. An artificially constructed unemployment indicator is used as the dependent variable (see page 222) and, given its discrete and dichotomous nature, probit regression models are employed in the analysis to estimate the predicted likelihood of being unemployed on the basis of the different regressors in each specification.

In the most basic specification (column 1), the BDH coefficient is negative and statistically significant (although its magnitude is quite small), indicating that having been affected by the conditional cash transfers slightly decreases the predicted probability of being unemployed. Using the cumulative distribution function (F) and holding all the other explanatory variables constant at zero, the predicted probabilities of being unemployed –for those individuals affected and not affected by the BDH cash transfers– are calculated as follows:

$$Probability = F(constant + \ BDH\ coef. * BDH\ indicator)$$
$$F(-2.6455 - 0.0721) = 0.0032 \ \ (affected\ by\ the\ BDH)$$
$$F(-2.6455) = 0.0040\ (not\ affected\ by\ the\ BDH)$$

Therefore, according to these results, individuals affected by the BDH are 0.08 percentage points less likely to be unemployed.

Moreover, the coefficient on the 2014-indicator is positive and significant (i.e. 0.1004). Thus, indicating that, holding everything else constant, in the post-treatment period individuals are about 0.14 percentage points more likely to be unemployed $[F(-2.6455 + 0.1004) - F(-2.6455)]$. On the other hand, the coefficient on the treatment group indicator is negative and significant (i.e. -0.3010), suggesting that individuals in the treatment group are 0.24 percentage points less likely to be unemployed $[F(-2.6455) - F(-2.6455 - 0.3010)]$.

The estimated coefficients do not vary much when household's average educational attainment is included in column 2. Specifically, the statistical significance and direction of the coefficients remain unchanged, while their magnitudes are a little smaller. However, once expressed in terms of probability the effects of the BDH, 2104-indicator and treatment group indictor are almost the same. This is true also for the following specifications (columns 3 and 4) when household's quality of floor material and age are included in the analysis.

Once again, these results are consistent with those obtained through the natural experiment. The main difference is that the impact of the BDH programme is much smaller when taking into account the trends over time and, above all, the unobserved characteristics of the groups. In other words, the diff-in-diff estimates show that although conditional transfers reduce somewhat the probability of being unemployed, the effect is much smaller than estimated by the previous method. Therefore, an important part of the BDH effect is explained simply by the intrinsic differences between the treatment and control groups.

In recent years there has been a broad debate about whether the benefits of social assistance increase the unemployment rate or not. These results show that, at least in the Ecuadorian case, the BDH programme of conditional transfers does not negatively affect the labour supply of the beneficiaries. This is contrary to the ideas present in academic and political spheres that indicate that welfare programmes are an important source of persistently high unemployment rates (Ghayad and Dickens, 2012). Therefore, these

results provide interesting evidence for the discussion, since they are special in the sense that determine the effects on the probability of unemployment of a CCT programme different from those that imply benefits only for unemployed (e.g. unemployment insurance extensions). In this way, it is shown that welfare payments do not always reduce employment among those who can claim such payments. On the contrary, it seems that BDH payments somewhat increase employment (i.e. reduce the probability of unemployment).

6.4.5 Lessons from the Diff-in-Diff Results

Overall, the diff-in-diff results confirm that the BDH has had a small but significant negative effect on welfare among those in poverty. Specifically, the impact is concentrated on some of its components, such as the level of instruction and floor material. In other cases, as in the probability of school attendance and school enrolment of the first-born child, no type of effect of the BDH is found. Finally, the impact of the BDH on unemployment appears to be slightly negative. More specifically, the results indicate that the BDH transfers somewhat reduce the likelihood that individuals will be without a stable job. Once again, the estimated results imply that the programme does not achieve almost any of the expected objectives previously described as part of the BDH's theories of change (see page 184). Therefore, it is quite probable that the resources, activities, mechanisms (e.g.. conditions) and channels of change are not being sufficient to reach high levels of efficiency or are not being implemented correctly.

As in the natural experiment, these results are inconsistent with the paternalistic idea that cash transfer programmes are effective when they are accompanied by a set of specific requirements related to desirable behaviours among those in poverty (i.e. when they are linked to conditions associated to human capital accumulation). Therefore, my diff-in-diff findings are also not contributing evidence in support of CCTs as the ideal type of welfare programmes in developing countries as discussed earlier in Chapter 1. On the

other hand, these results seem consistent with the idea that poverty is caused by the very efforts to alleviate it (i.e. welfare dependency or welfare trap theory) and even with the notion that simply giving money to the poor -no conditions attached- may be a better option in terms of poverty reduction (i.e. rational choice theory).

However, we must be cautious when interpreting the theoretical implications of these results, since the fact that a traditional cash transfer programme does not have the expected results in Ecuador does not necessarily mean that any type of social assistance could actually be useful in this country, nor does it imply that these findings can be applied to other cases or countries. For example, it could be happening that the estimated negative effects are more associated with the implicit conditionalities rather than the cash transfers themselves. If this is the case, other types of programmes, which rely on the rationality of individuals and on trusting their own capacity to invest appropriately the available monetary resources, could present better results for people living in poverty (i.e. rational choice theory).

In fact, unconditional cash transfer (UCT) programmes have their own advocates in the academic circles and also a large amount of empirical evidence supporting them (see Chapter 1, page 37). Thus, this awakens the notion that cash transfers with no conditions attached may be a better option in terms of poverty reduction for the Ecuadorian context. This of course will be properly evaluated in an upcoming chapter on the effects of the CDH programme, which in an innovative way presents different characteristics to the traditional CCT programmes.

One important advantage of this thesis is that it analyses the effects of a traditional cash transfer programme taking into account a large part of the population of individuals in poverty situation at two different points of time (i.e. 2008 and 2014). Hence, in this case, it can be somewhat difficult to determine the type of treatment effect that is actually identified through the difference-in-differences strategy. In fact, the diff-in-diff approach as used in this thesis would in theory allow the estimation of two different types of treatment effect: the average treatment effect in the population (ATE) and the average

treatment effect on the treated (ATT). It all depends on the characteristics of the sample and, more specifically, on how close we think the selected sample is to encompass a substantial portion (if not all) of the population. In order to better understand the difference between both types of average causal effects, as well as where exactly the effects estimated on this thesis lie, it is important to outline a short explanation of their concepts and main assumptions in the context of the present study.

Excluding the realm of science fiction –where parallel universes are sometimes imagined to be observable– it is not possible to calculate individual-level causal effects, because no individual can be in two different states of participation at the same point in time. In other words, the fundamental evaluation problem can be resumed in that we do not observe both possible outcomes, y_i^1 and y_i^0, for anyone. Thus, it is impossible to construct person-specific impact estimates just by looking at observational data. If we could construct these impacts, then we would simply have to aggregate them up to obtain any parameter we would like. As a result, instead of constructing individual impacts, researchers have focused on average causal effects.

In order to further clarify the idea behind an average causal effect, recall that we are interested in the effects of the BDH cash transfer programme on the general well-being of poor individuals. Let i index individuals or the number of units, $i = 1,2, \ldots N$. The binary "treatment" indicator is D_i, where $D_i = 1$ denotes "affected by the BDH". For each population unit, there are two possible outcomes: y_i^1 denotes the potential well-being of individual i if he was affected by the monetary transfers (the outcome with treatment) and y_i^0 denotes the potential well-being of individual i if not (the outcome without treatment). As stated above, both cannot be observed at the same time. Thus, one of them will be "counterfactual": an outcome that would have occurred if a different treatment had been given. While, on the other hand, the observed outcome for individual i is given by

$$y_i = D_i y_i^1 + (1 - D_i)\, y_i^0$$
$$= y_i^0 + D_i(y_i^1 - y_i^0).$$

While the unit level treatment effect is formally written as

$$\beta_i = y_i^1 - y_i^0$$

We are interested specifically in $y_i^1 - y_i^0$, the difference of the well-being outcome with and without BDH treatment, an object which is clearly not possible to identify. However, there are some moments in its distribution (because this difference is a random variable) that we are interested in estimating, which can be identified under certain conditions. Some of the most widely studied moments in the treatment effects context are the average treatment effect (ATE), and the average treatment effect on the treated (ATT).

An average treatment effect (ATE) parameter is the expected partial effect for a binary explanatory variable on a randomly selected individual from the population (Wooldridge, 2010). Sometimes called the average causal effect, it is given by

$$ATE = E(y_i^1 - y_i^0)$$

In the context of this thesis, the ATE parameter responds, for example, to the question of what would be the mean impact of the BDH programme if this type of cash transfers were delivered to the entire population of people living in poverty. Therefore, the underlying assumption is that the estimated effect of the treatment is the same for all poor individuals in Ecuador. This is the case even when the actual effect of the social assistance programme for some individuals may actually be very different from the average effect in that population. Overall, it can be interpreted as a causal effect in the sense that it is the best predictor of the treatment impact without further information.

The estimation of an ATE requires addressing the fundamental problem of causal inference in observational studies (i.e. missing data due to counterfactual outcomes). In order to overcome this issue in this thesis, I compared a group of individuals exposed to the treatment with a non-experimental comparison or control group that was not exposed

to it. However, because of the potential selection bias, another important assumption required to estimate an ATE is the presence of "ignorable treatment assignment" (which means that the treatment and control groups are randomly drawn from the population) conditional on the observed covariates (X_i)[85]:

$$(y_i^1,\ y_i^0) \perp D_i \mid X_i$$

Therefore, relating these concepts and assumptions to the intended estimations by this thesis, the ATE would be defined as the expected effect of participation in the BDH cash transfer programme for a randomly selected member of the *entire* population under study; while, on the other hand, the average treatment effect on the treated (ATT) would be the mean effect for a randomly drawn individual from those who actually received the cash transfers:

$$ATT = E(y_i^1 - \ y_i^0 \ |D_i = 1)$$
$$ATT = \ E(y_i^1 \ | \ D_i = 1) - E(y_i^0 \mid D_i = 1)$$

Despite having similar interpretations, these types of treatment effects have fundamental conceptual differences. In contrast to the ATE, the ATT answers the question of how do the BDH cash transfers change the well-being of those poor individuals affected by this type of treatment, compared to what they themselves would have experienced if they had not participated in the social assistance programme. Note that the last expression highlights the counter-factual nature of a causal effect. The first term, $E(y_i^1 \mid D_i = 1)$, is a potentially observable quantity; while on the other hand, the second term, $E(y_i^0 \mid D_i = 1)$, cannot be observed. However, we can again use a non-experimental control group and condition on a set of pre-treatment characteristics (observed covariates X_i) such that:

$$E(y_i^1 - \ y_i^0 \ |D_i = 1, X_i) = E(y_i^1 \ |D_i = 1, X_i) - E(y_i^0 \ |D_i = 0, X_i)$$

[85] See Brand and Halaby (2003) pp.10.

Hence, the ATT can be consistently estimated under a much weaker assumption than the one of "ignorable treatment assignment" (i.e. random selection), which was necessary for the ATE. Namely, the ATT is obtained under the assumption that the outcome without treatment (y_i^0) is independent of being affected by the BDH cash transfers (D_i), conditional on the observed covariates:

$$y_i^0 \perp D_i \mid X_i$$

This simple assumption allows the use of a control group that is selected in such a way that it resembles the treated subjects as much as possible, which implies that the selection is not necessarily random. Nevertheless, it is important to note that if the stronger conditional statement $[(y_i^1, y_i^0) \perp D_i \mid X_i]$ is true, then the ATT should be equal to the ATE. Generally, the ATE and ATT differ and only in some special cases they coincide (Brand & Halaby, 2003).

I strongly believe that the identification strategy employed in this thesis allows the estimation of the average treatment effect (ATE). This means that the estimated effects constitute the mean impacts of the BDH cash transfers for a randomly selected individual from the entire (or almost entire) target population, which is composed of working-age adults in poverty and without disabilities. In fact, it was stated from the beginning that poor households were randomly chosen to be part of the programme by BDH administrators. That is, the households that received the transfers at least once until 2014 were not chosen in a systematic manner or based on observable characteristics, once they met the fundamental requirement of being in the first two quintiles of the poverty index (i.e. being considered poor). This was effectively verified by comparing the characteristics of the treatment and control groups in 2008 prior to the intervention (see page 191). Therefore, the presence of "ignorable treatment assignment" is sufficiently credible since there is evidence supporting that the groups were randomly drawn from the population.

It is important to mention at this point that difference-in-differences estimates are usually considered to be average treatment effects on the treated (ATT), rather than

average treatment effects (Ryan et *al*., 2015). This is because diff-in-diff estimates are typically thought of as applying exclusively to a particular group that was treated or intervened (rather than to a population that could have been treated). However, in this case, the analysed sample encompass a significant portion of the target population, which is made up of only potential beneficiaries of the BDH programme and who, therefore, could have been treated.

Moreover, beneficiaries were randomly selected since there is no other requirement than being poor to receive the BDH cash transfers. Finally, individuals in the control group were not selected to resemble the treated subjects at all. Actually, they were all included in the analysed sample as long as they meet the characteristics of the target population. Thus, there are sufficient grounds to claim that the treatment effects found in this thesis are, in fact, average treatment effects (ATE).

The results obtained show that the current effectiveness of the BDH conditional transfer programme to improve welfare conditions and boost the investment in human capital of the poorest would be, at least, below its potential. Although one of the objectives of social assistance programmes in the short term is the reduction of poverty by income, in the long term these seek to improve the living conditions of people, mitigate social risks that affect human capital and reduce unemployment providing poor families with the necessary tools to be self-sufficient.

In this sense, the effectiveness of the BDH would not be fulfilling its expected objectives since the estimated effects - in this case on the welfare index (and its main components), enrolment and school attendance, and unemployment levels - suggest that the families that receive cash transfers are not in a better situation than those households that do not receive these benefits (in fact, they could be worse off). These results could easily be associated with the lack of control and monitoring of conditionalities, but it is much more likely that this type of programme is not really the most adequate to achieve the desired objectives, since no control mechanism would be sufficient to ensure the

fulfilment of any condition if the beneficiaries are not convinced of their importance or if they do not have sufficient human and financial resources to send their children to school.

Table 21: Diff-in-Diff Regression Estimates of the Effect of the BDH on the Welfare Index

(Standard Errors in Parentheses)

	OLS	OLS	OLS	OLS	OLS
Independent variable	(1)	(2)	(3)	(4)	(5)
Treatment Group Indicator	-0.9700*** (0.0187)	-0.6475*** (0.0179)	-0.4669*** (0.0161)	-0.4627*** (0.0161)	-0.4670*** (0.0160)
2014 Year Indicator	10.6254*** (0.0157)	10.1701*** (0.0150)	8.5521*** (0.0138)	8.5507*** (0.0138)	8.1733*** (0.0139)
BDH Effect (Treatment Group Indicator * 2014 Year Indicator)	-1.2370*** (0.0258)	-0.9314*** (0.0246)	-0.8560*** (0.0222)	-0.8548*** (0.0222)	-0.8653*** (0.0221)
Household's Educational Attainment	---	2.3956*** (0.0053)	1.9722*** (0.0049)	1.9704*** (0.0049)	2.1729*** (0.0050)
Household's Floor Material	---	---	3.1586*** (0.0046)	3.1584*** (0.0046)	3.1679*** (0.0046)
Unemployment Indicator	---	---	---	0.6509*** (0.0493)	0.8041*** (0.0490)
Age	---	---	---	---	0.0777*** (0.0005)
Constant	14.8845*** (0.0406)	10.1988*** (0.0401)	0.3796*** (0.0390)	0.3824*** (0.0390)	-2.5928*** (0.0433)
Reported Province of Residence Dummies	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓
Number of Observations	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708
R-squared	0.3493	0.4080	0.5185	0.5185	0.5241

Note: Estimates are from linear regression models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)

Table 22: Household's Educational Attainment Diff-in-Diff Regressions (BDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
	Dependent Variable: Household's Average Educational Attainment							
Treatment Group Indicator	-0.2241***	0.7992***	-0.2108***	0.8098***	-0.2077***	0.8124***	-0.2064***	0.8134***
	(0.0039)	(0.0031)	(0.0039)	(0.0031)	(0.0039)	(0.0031)	(0.0039)	(0.0032)
2014 Year Indicator	0.2310***	1.2598***	0.1255***	1.1338***	0.1247***	1.1328***	0.3398***	1.4047***
	(0.0033)	(0.0041)	(0.0033)	(0.0038)	(0.0033)	(0.0038)	(0.0034)	(0.0048)
BDH Effect	-0.1754***	0.8390***	-0.1708***	0.8429***	-0.1701***	0.8435***	-0.1638***	0.8488***
(Treatment Group Indicator * 2014 Year Indicator)	(0.0054)	(0.0046)	(0.0055)	(0.0046)	(0.0055)	(0.0046)	(0.0055)	0.0047
Household's Floor Material	---	---	0.2058***	1.2286***	0.2057***	1.2284***	0.1962***	1.2168***
			(0.0011)	(0.0014)	(0.0011)	(0.0014)	(0.0011)	(0.0014)
Unemployment Indicator	---	---	---	---	0.4486***	1.5661***	0.3416***	1.4073***
					(0.0121)	(0.0189)	(0.0121)	(0.0171)
Age	---	---	---	---	---	---	-0.0472***	0.9538***
							(0.0001)	(0.0001)
Number of Observations	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 23: Household's Quality of Floor Material Diff-in-Diff Regressions (BDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
Dependent Variable: Household's Quality of Floor Material								
Treatment Group Indicator	-0.1344***	0.8741***	-0.1044***	0.9008***	-0.1038***	0.9013***	-0.1042***	0.9010***
	(0.0039)	(0.0034)	(0.0039)	(0.0035)	(0.0039)	(0.0035)	(0.0039)	(0.0035)
2014 Year Indicator	0.9146***	2.4959***	0.8841***	2.4210***	0.8839***	2.4205***	0.9015***	2.4633***
	(0.0034)	(0.0085)	(0.0034)	(0.0083)	(0.0034)	(0.0083)	(0.0035)	(0.0086)
BDH Effect	-0.0775***	0.9253***	-0.0500***	0.9511***	-0.0498***	0.9513***	-0.0493***	0.9518***
(Treatment Group Indicator * 2014 Year Indicator)	(0.0055)	(0.0051)	(0.0055)	(0.0052)	(0.0055)	(0.0052)	(0.0055)	(0.0052)
Household's Educational Attainment	---	---	0.2353***	1.2653***	0.2351***	1.2650***	0.2254***	1.2529***
			(0.0012)	(0.0015)	(0.0012)	(0.0015)	(0.0012)	(0.0016)
Unemployment Indicator	---	---	---	---	0.0941***	1.0987***	0.0867***	1.0906***
					(0.0125)	(0.0137)	(0.0125)	(0.0136)
Age	---	---	---	---	---	---	-0.0036***	0.9963***
							(0.0001)	(0.0001)
Number of Observations	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708	1,989,708
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 24: Diff-in-Diff Regression Estimates of the Effects of the BDH on School Enrolment and School Attendance of the First-born Child

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)	Probit Regression (5)
Dependent Variable: First-born Child Enrolled in School Dummy					
Treatment Group Indicator	-0.0350***	-0.0011	0.0014	0.0013	0.0022
	(0.0058)	(0.0060)	(0.0060)	(0.0060)	(0.0060)
2014 Year Indicator	0.0751***	-0.1165***	-0.1362***	-0.1362***	-0.087***
	(0.0060)	(0.0063)	(0.0064)	(0.0064)	(0.0065)
BDH Effect (Treatment Group Indicator * 2014 Year Indicator)	-0.0114	-0.0077	-0.0065	-0.0066	-0.0173*
	(0.0097)	(0.0102)	(0.0102)	(0.0102)	(0.0102)
Household's Educational Attainment	---	0.5639***	0.5586***	0.5586***	0.5461***
		(0.0030)	(0.0030)	(0.0030)	(0.0031)
Household's Floor Material	---	---	0.0388***	0.0388***	0.0370***
			(0.0019)	(0.0019)	(0.0019)
Unemployment Indicator	---	---	---	-0.0172	-0.0286
				(0.0212)	(0.0212)
Age	---	---	---	---	-0.010***
					(0.0002)
Constant	0.4426***	-0.7027***	-0.8225***	-0.8225***	-0.343***
	(0.0126)	(0.0145)	(0.0157)	(0.0157)	(0.0203)
Number of Observations	430,931	430,931	430,931	430,931	430,931
Dependent Variable: First-born Child Attending School Dummy					
Treatment Group Indicator	-0.0349***	-0.0018	0.0009	0.0008	0.0019
	(0.0057)	(0.0059)	(0.0059)	(0.0059)	(0.0060)
2014 Year Indicator	0.0062	-0.1867***	-0.2081***	-0.2081***	-0.159***
	(0.0058)	(0.0062)	(0.0062)	(0.0062)	(0.0064)
BDH Effect (Treatment Group Indicator * 2014 Year Indicator)	-0.0107	-0.0073	-0.0061	-0.0061	-0.0171*
	(0.0095)	(0.0099)	(0.0099)	(0.0099)	(0.0099)
Household's Educational Attainment	---	0.5402***	0.5344***	0.5344***	0.5221***
		(0.0030)	(0.0030)	(0.0030)	(0.0030)
Household's Floor Material	---	---	0.0420***	0.0420***	0.0402***
			(0.0019)	(0.0019)	(0.0019)
Unemployment Indicator	---	---	---	-0.0117	-0.0232
				(0.0209)	(0.0209)
Age	---	---	---	---	-0.010***
					(0.0002)

Constant	0.4086*** (0.0125)	-0.6910*** (0.0143)	-0.8209*** (0.0154)	-0.8209*** (0.0154)	-0.340*** (0.0200)
Number of Observations	430,931	430,931	430,931	430,931	430,931
Province of Residence Dummies	✓	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)
* Significant at the 10 percent level (P value<=0.1)

Table 25: Unemployment Diff-in-Diff Regressions (BDH)

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)
Dependent Variable: Dummy for Being Unemployed				
Treatment Group Indicator	-0.3010*** (0.0092)	-0.2942*** (0.0092)	-0.2934*** (0.0092)	-0.2852*** (0.0093)
2014 Year Indicator	0.1004*** (0.0061)	0.0798*** (0.0061)	0.0743*** (0.0063)	0.1168*** (0.0065)
BDH Effect (Treatment Group Indicator * 2014 Year Indicator)	-0.0721*** (0.0123)	-0.0615*** (0.0123)	-0.0614*** (0.0123)	-0.0638*** (0.0124)
Household's Educational Attainment	---	0.0853*** (0.0021)	0.0840*** (0.0021)	0.0683*** (0.0022)
Household's Floor Material	---	---	0.0107*** (0.0023)	0.0097*** (0.0023)
Age	---	---	---	-0.0078*** (0.0002)
Constant	-2.6455*** (0.0195)	-2.8203*** (0.0201)	-2.8557*** (0.0215)	-2.5750*** (0.0236)
Number of Observations	1,989,708	1,989,708	1,989,708	1,989,708
Province of Residence Dummies	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)

CHAPTER 7

Crédito de Desarrollo Humano Impact Evaluation Design and Measurement Models

7.1 Introduction

The time has come to lay the foundations for the various impact evaluation methods to be used for the Crédito de Desarrollo Humano (CDH) programme. In the same way that it was done for the other programme, this chapter commences with a theory-based approach to the CDH evaluation that allows the description of the steps that lead to the long-term objectives of the programme, and the connections between these activities. The Theories of Change of the CDH programme are represented in a diagram, which is then used to design the evaluation. This is followed by an explanation of the sample selection process and a review of the pre-treatment characteristics of the sample groups (i.e. CDH treatment and control). Finally, the last part of the chapter specifies the measurement models for data analysis, including the identification strategies, the econometric techniques, and the specific regression models to be estimated as part of the CDH evaluation.

7.2 Theory of Change of the Crédito de Desarrollo Humano

In order to guide the impact evaluation of the CDH cash transfer programme, it is necessary to build an outcomes framework, which is the final product of the Theory of Change (ToC) methodology and describes the different channels of change through which the desired results are expected to be obtained. Remember that, using the ToC methodology, it is possible to evaluate to what extent the intervention has the potential to contribute to achieving the desired results. All the information necessary for the development of the outcomes framework of the CDH programme comes from different official sources. Several public institutions such as the Instituto de Economia Popular y Solidaria (IEPS), the Ministerio Coordinador de Desarrollo Social (MCDS) and the Ministerio de Inclusión Económica y Social (MIES) have developed reports and bulletins to inform on the process of implementation of the programme, as well as its expected objectives, activities and the necessary pre-conditions for its successful operation (IEPS, 2014, MIES, 2013b, MCDS, 2010). In addition, the Inter-American Development Bank (IDB) prepared a technical note that explains, at least partially, how this programme works in Ecuador (Martinez et al., 2017). The CDH outcomes framework is presented in Figure 18.

First, it is necessary to identify the specific aims and objectives of the CDH programme and then project them backwards, going through the mechanisms and activities of the intervention, to finally identify all the necessary preconditions that it is assumed must be in place for the goals to be met. The objectives and expected results of the CDH programme were already discussed in Chapter 2 (see page 89). The main goal is to improve the quality of life among the poorest families in the country by providing access to non-reimbursable microcredits for them to invest in productive activities that promote their economic capacity (IEPS, 2014). These and other specific programme objectives are summarized in the outcomes framework.

The channels of change through which the theoretical objectives of the programme are expected to be achieved are basically changes in the availability of productive credit, personal values and attitudes (IEPS, 2013). For example, the value that families place on knowledge (i.e. human capital) is important for them to be willing to acquire the necessary information to make good investments with their money. Only if poor households improve their ability to recognize investment opportunities, they will be able to take full advantage of the accumulated cash transfers and change the way they do business.

Therefore, apart from the productive credit itself, the most important mechanisms to promote these changes are: *trust* in the poor and the knowledge that should be transmitted to them. Trust is a highly valued moral good and an essential element of all types of economic transactions, without it no bank (or government) would be willing to provide considerable amounts of capital to invest in productive activities and little commercial activity would be possible (Fukuyama, 1995; Knack & Keefer, 1997; Boatright, 2011). In this case, trust is essential to satisfy the credit needs of poor families that almost always lack collateral, certifiable credit history or material goods. It is precisely because of the usual lack of confidence in them by private banks that poor households are often forced to accept high interest rates from local credit cooperatives or even resort to unscrupulous usurers to raise capital to invest in their small businesses.[86] In addition, the other key mechanism is the possession and exposure to knowledge, which is arguably an effective way to increase the likelihood of engaging in business start-up activity and minimize the risks of any productive investment (De Clercq & Arenius, 2006; West & Noel 2009).

The main activities or initiatives theoretically promoted by the CDH programme –which are of course linked to the mechanisms of change– are among others: the provision of lump-sum unconditional cash transfers, the availability of technical assistance and expert advice for the beneficiaries, the requisition of a savings account for the deposit of

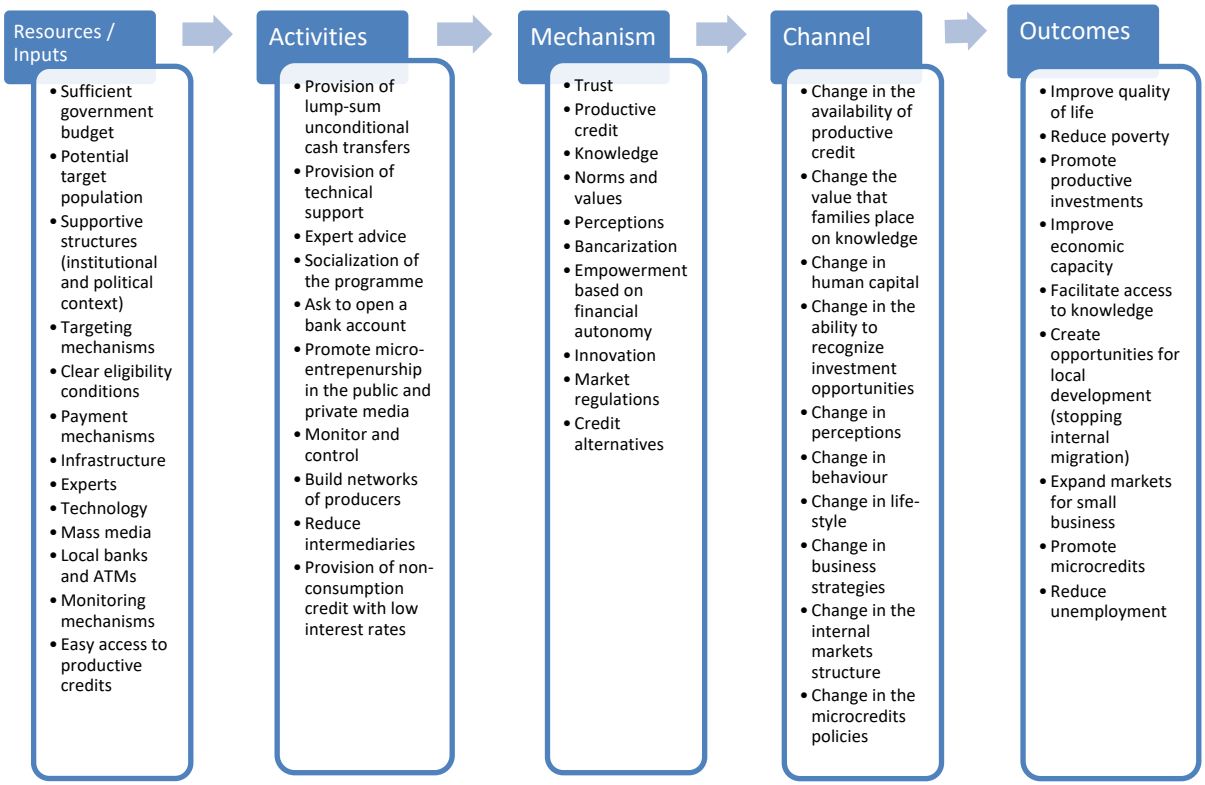
[86] See Boatright (2011) for an interesting discussion on the role played by trust in banking and Fukuyama (1995) for an analysis of the importance of trust in the development of social capital.

the transfer, the promotion of micro-entrepreneurship as a desirable practice that bears fruit when it is done responsibly, the monitoring and control of the results obtained by the investment initiatives to offer solutions and new opportunities, and the generation of social networks to take advantage of economies of scale and reduce the presence of intermediaries (Martinez et al., 2017; MCDS, 2010).

Finally, the ToC methodology allows the identification of the necessary preconditions for the CDH programme to achieve all its objectives. Some of these are the same as those previously identified for the Bono de Desarrollo Humano (BDH) programme, such as having efficient targeting, payment and monitoring mechanisms (MIES, 2013b). However, this programme requires other resources that are perhaps a little more difficult to satisfy for the administrators, such as the right amount of people qualified to provide technical advice. For this innovative initiative to be successful, experts should be capable of meet the demand for assistance from the beneficiaries. In addition, an efficient consulting service requires adequate infrastructure (e.g. buildings, materials, books, etc.), technology (e.g. internet, computers, etc.) and people. Last but not least, given that the transfers are deposited in savings accounts, it would be important to provide the beneficiaries of banks and ATMs that are close to their locations (MCDS, 2010).

Similar to what was done in the previous chapter, once the effects of the CDH have been estimated using different econometric techniques, they will be compared with the desired results of the programme (that is, those expected to be obtained) to assess whether the ‘change dynamics’ are in fact functioning as smoothly and efficiently as the CDH theories of change indicate.

Figure 18: Outcomes Framework of the CDH Programme

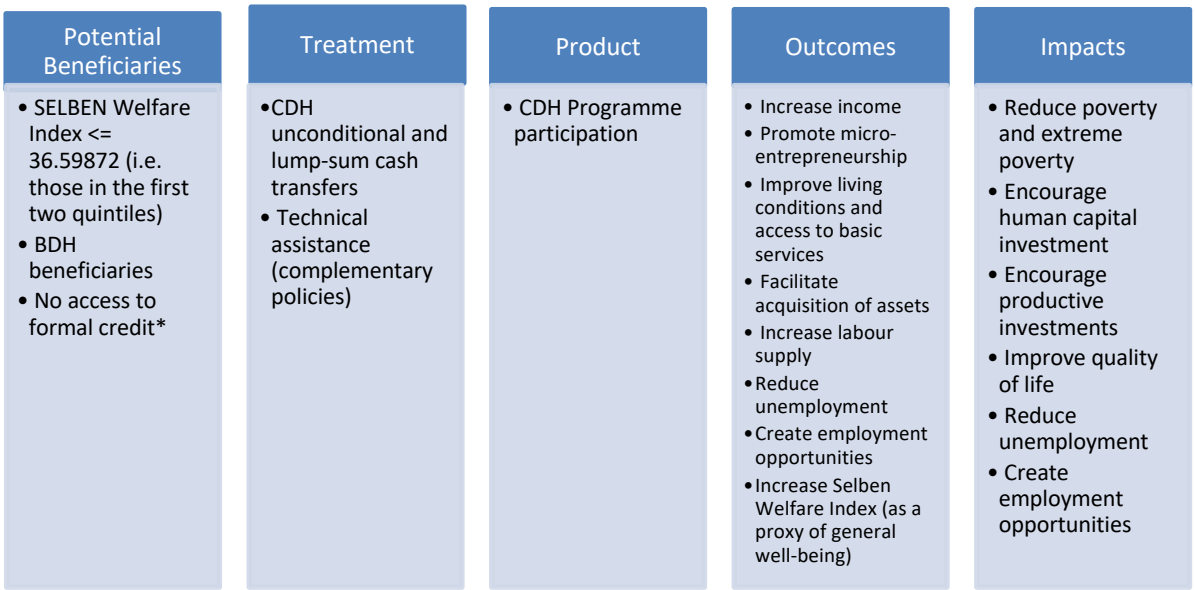


7.3 Evaluation Design

Now, in order to make a correct assessment of the impacts of the CDH programme on poverty and well-being –once 6 years have passed since its implementation (from 2008 to 2014)–, it is essential to identify the *potential beneficiaries* of the cash transfers. Then, it is important to define the specific *treatment* given to those individuals who, in this case, self-selected to participate in the initiative and the *products* arising from this type of treatment. Finally, it is required to determine the expected *results* and *impacts* of the programme. All this is represented through an evaluation design/scheme that summarizes each of the components that lead to the process of change (see Figure 19).

The potential beneficiaries of the CDH cash transfers are *only* those poor households (i.e. in the first two quintiles of the Welfare Index) who are already beneficiaries of the BDH programme, and who typically have no access to formal credit (mainly due to the absence of transaction history and proof of stable income for assessment by banks). Note that the lack of access to formal credit is not a requirement to participate in the programme, but a characteristic that is normally shown by the beneficiaries of both programmes. The treatment itself is that the person gets a lump-sum unconditional payment or not, so the product thereof is to participate in the CDH social assistance programme. The expected results, described here in a more detailed way, are closely related to the desired impacts of the programme: reduce poverty and extreme poverty, improve the quality of life, encourage productive investments, reduce unemployment, etc.

Figure 19: Evaluation Design for the CDH Programme



7.4 Sample Selection Process

The process of selecting the sample used for the evaluation of the CDH programme starts once again from the panel-type database that was artificially constructed with the Registro Social 2008 and 2014. Recall that this database is made up of more than 3 million people with different socioeconomic characteristics. Therefore, it is necessary to limit the sample (so that it is homogeneous) considering only those households that are part of the target population of the intervention (i.e. families in a situation of poverty). In addition, individuals in the sample must meet the single eligibility condition of the CDH programme (i.e. to be already beneficiaries of the BDH cash transfers) and must not be part of other social groups considered vulnerable (i.e. elderly or with disabilities). Specifically, only individuals with all of the following characteristics are taken into account:

- ✓ Individuals in poverty situation according to the SELBEN Welfare Index.
- ✓ BDH beneficiaries reported in 2014 (including those who chose the CDH programme at some point and those who did not).
- ✓ Working-age adults (individuals who have at least 18 years old in 2008 and at most 65 years old in 2014)
- ✓ Individuals without disabilities (neither in 2008 nor in 2014)

By including in the sample only those who reported in 2014 having received (or continue receiving) BDH cash transfers, one can almost guarantee that the sample is made up of people living in poverty and, more importantly, that it is entirely composed of *potential* beneficiaries of the CDH programme. That is, individuals who at least had the possibility to self-select themselves for the accumulated cash transfers. Note that only by limiting the sample in such a way is it possible to estimate the specific effects of the CDH programme, since if we include in the sample –as the control group– those individuals who did not receive any type of cash transfer (i.e. the non-beneficiaries), the estimated impact would actually be that of the combination of both programmes.

Moreover, the elderly and people with disabilities should not be considered for this analysis, mainly because they are not necessarily in a situation of poverty. Even if they were, they also receive more cash transfers from other social assistance programmes, which are targeted on groups with a higher level of vulnerability (see Chapter 2, page 67). Consequently, it is not possible to isolate the effect of the BDH or CDH programmes on their level of well-being, which would complicate the interpretation of the results. In the end, this process leaves us with a total of 719,987 individual observations.

The selected sample could be defined as the group of those working-age individuals without disabilities who, being in a situation of poverty, reported having participated of the BDH cash transfers. This sample is divided into those BDH beneficiaries who opted (at least once) for the CDH programme and those who did not. As a result, the treatment and control groups are defined as follows:

- Treatment group: BDH beneficiaries who reported having participated at least once of CDH cumulative cash transfers until 2014.
- Control group: Individuals who reported having participated *only* of the BDH programme until 2014.

Table 26 shows the number of individuals in each sample group.

Table 26: CDH Evaluation Sample Groups

Groups	Number of Individuals
Control	385,371
CDH Treatment	334,616
Missing	0
Total	719,987

7.5 Pre-treatment Characteristics of the Sample

The identifying assumption of the natural experimental method is that the CDH treatment assignment can be considered as having been *randomized*. This assumption requires that the treatment status be independent of the potential outcomes Y (conditional on a set of covariates X). In a Randomized Control Trial (RCT), this assumption is trivially satisfied because everything will be independent of the assignment. However, in observational studies (like this one), it is important to check whether the selection of beneficiaries could be actually considered "as if" random.

Given that in this case, individuals self-select themselves to receive the accumulated cash transfers, a priori it is difficult to assume that the randomization condition is satisfied. The most probable is that exogeneity of treatment assignment does not hold and causal inference using standard methods (like the natural experiment) is likely to exhibit bias. Thus, alternative forms of quantitative evaluation that can more credibly claim exogeneity of treatment (such as instrumental variables and diff-in-diff methods) become necessary to apply.

In order to verify whether there is a balance between the treated and non-treated groups on pre-intervention characteristics, a base-line comparison should be performed. Table 27 presents descriptive statistics of the outcome variables (and covariates) by sample group in the pre-treatment period.

Table 27: CDH Pre-intervention Descriptive Statistics by Sample Group

	Control			CDH Treatment		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Welfare Index	22.035	8.170	385,371	22.031	7.355	334,616
Educational Attainment	2.433	1.041	385,371	2.585	1.004	334,616
Floor Material	3.956	1.182	385,371	3.988	1.120	334,616
School Enrolment	0.773	0.418	102,377	0.807	0.394	93,077
School Attendance	0.768	0.421	102,377	0.802	0.398	93,077
Unemployment	0.007	0.083	385,371	0.007	0.088	334,616

In general, we can see from Table 27 that the sample groups were quite homogenous before the implementation of the CDH programme. All variables considered in this descriptive statistics analysis have similar mean values for treatment and control groups. This is evident especially in the Welfare Index, the quality of floor material and the likelihood of unemployment. The average probabilities of school enrolment and attendance are fairly similar in both groups, however there is a very small difference in favour of the CDH beneficiaries. Something similar is observed with the average educational attainment of households. In this case, the difference between the groups is somewhat greater, but not enough to consider that the CDH beneficiaries are much superior in this regard and, therefore, that their decision to opt for the accumulated transfers is explained *only* by their higher educational level.

Surely there must be other unobserved characteristics that explain the self-selection decisions of the individuals and that also affect the outcome variables. However, at least in terms of these variables, the descriptive statistics indicate that the natural experimental method can be quite useful for estimating the effects of the CDH. Other quasi-experimental methods will be important to address potential endogeneity and omitted-variable problems, which may arise as a result of self-selection. The small pre-intervention differences between the treatment and control groups can be better noted by plotting the means of the outcome variables (Figures 20 to 25).

Figure 20: Mean of Welfare Index in 2008

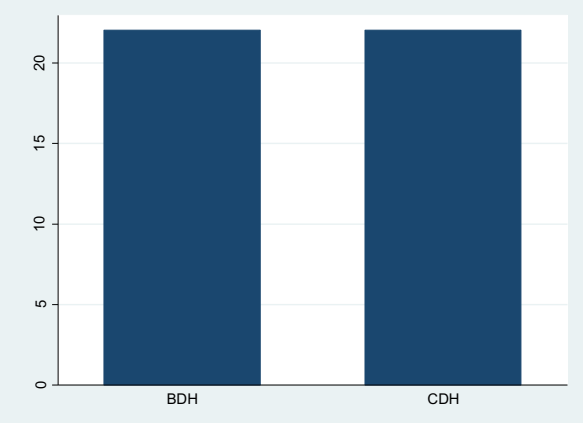


Figure 21: Mean of Households' Educational Attainment in 2008

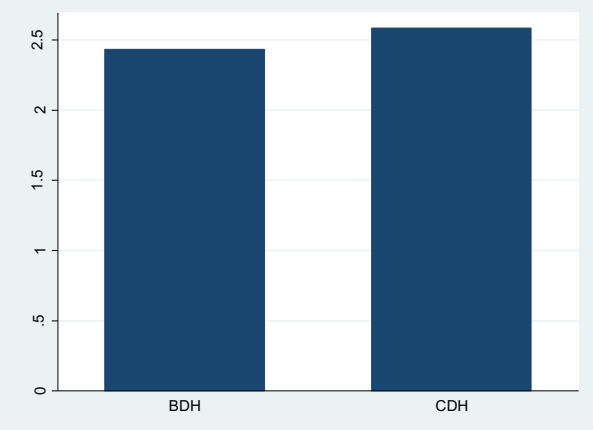


Figure 22: Mean of Households' Quality of Floor Material in 2008

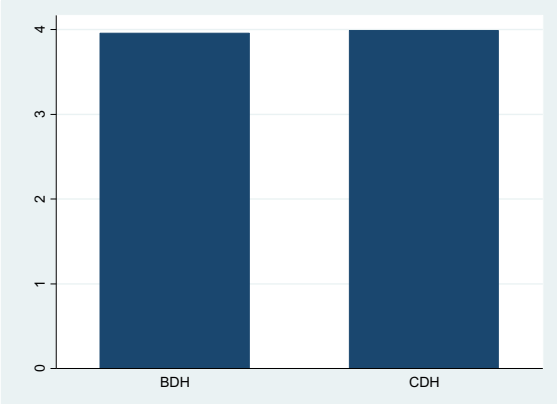


Figure 23: Mean Probability of School Enrolment in 2008 (First-born Child)

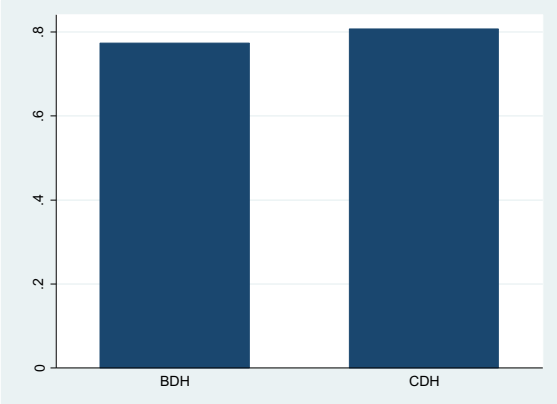


Figure 24: Mean Probability of School Assistance in 2008 (First-born Child)

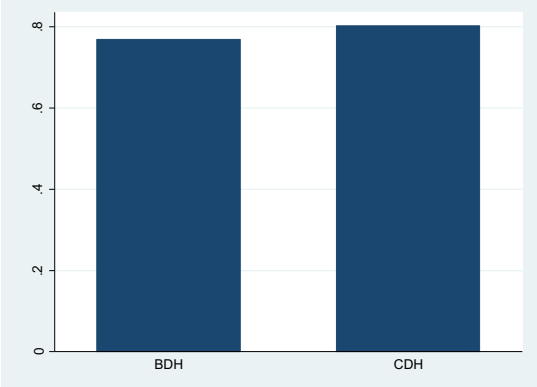
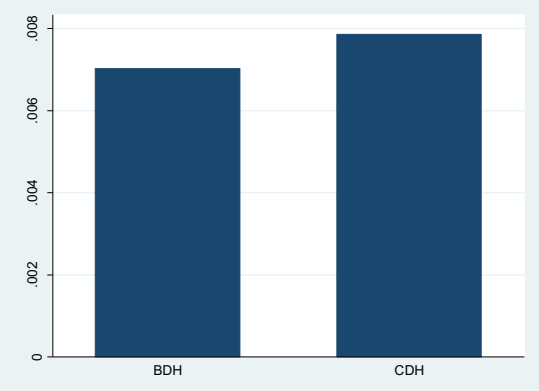


Figure 25: Mean Probability of Unemployment in 2008

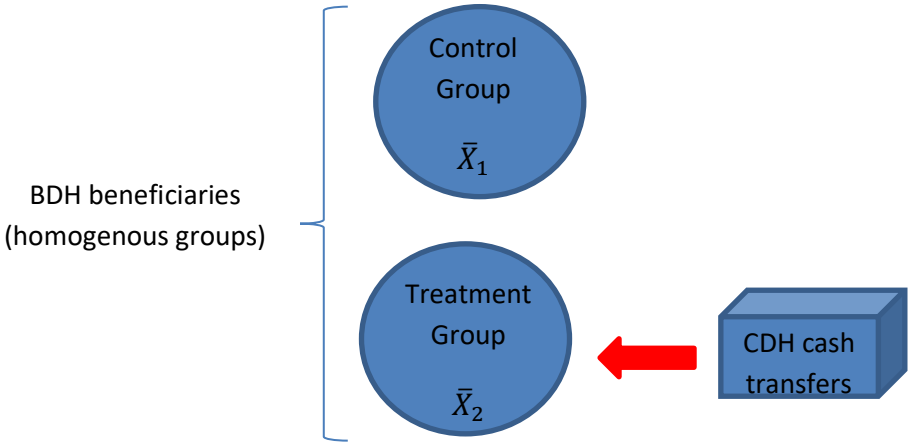


7.6 Measurement Models for Data Analysis

Even when it is likely that the assignment of CDH cash transfers was not entirely random or as-if random due to self-selection of beneficiaries, one can still get useful estimates of the treatment effects by using a natural experimental design. The homogeneity of the sample groups prior to the intervention supports the idea that perhaps there are no important omitted factors that affect both the treatment assignment and the outcome variables (i.e. endogeneity). Besides, this will serve as a preliminary step to the implementation of more sophisticated quantitative methods that obtain more reliable estimates by addressing the potential endogeneity problems (i.e. instrumental variable and difference-in-differences methods).

As was done for the evaluation of the BDH programme, I start by using a simple difference-of-means test and multivariate regression model analysis. These two econometric techniques are part of the natural experimental design proposed in this thesis as a first method of evaluation. Figure 26 illustrates graphically the natural experiment proposed for the evaluation of the CDH programme.

Figure 26: Natural Experimental Design (CDH)



7.6.1 Difference-of-means Test

In this case, two independent groups of beneficiaries of the BDH programme were chosen and, therefore, it is expected that they have alike observable characteristics prior to the implementation of the CDH initiative (i.e. in 2008).[87] Individuals who have been self-selected to receive CDH cash transfers -at some point between 2009 and 2014- constitute the treatment group, and the others who have not received them are the control group. As it was done in the evaluation of the BDH, a set of dependent variables is chosen on which I want to know if the CDH treatment has had a statistically significant effect.

If the two previously homogeneous groups of beneficiaries and non-beneficiaries of the CDH programme have similar means in the dependent variables after the treatment (i.e. in 2014), this would mean that receiving accumulated cash transfers beyond BDH has not made any difference in their current poverty situations.

As in every difference-of-means test, it is required to state a null hypothesis and an alternative hypothesis. Thus, the null hypothesis will be that there is no difference

[87] This was already examined and probed in the previous section as part of the sample selection process.

between the means of the CDH treatment group and the control group. Both hypotheses are formally expressed as follows:

$$H_0: \mu_1 = \mu_2 \ (i.e. there\ is\ no\ CDH\ effect)$$

$$H_a: \mu_1 \neq \mu_2 \ (i.e. the\ CDH\ has\ an\ effect)$$

Note that μ represents the means of the dependent variable for the treatment and control groups and that this two-sample t-test will use a significance level equal to 0.05.

7.6.2 Multivariate Regression Model

In order to obtain the effects of the CDH programme on different outcome variables (Y), I estimate several versions of the following standard regression model via ordinary least squares (OLS), probit or logit regressions:

$$Y_i = \alpha + \gamma CDH_i + \sigma X_i + \varepsilon_i \ , \qquad (7)$$

Where Y is any of the outcomes under study, and γ is the parameter of interest, which captures the causal effects of the *CDH* cash transfers. The CDH indicator is constructed using a dummy variable that equals 1 for those BDH beneficiaries self-selected to be part of the CDH, and 0 otherwise. X is a vector of individual- and household-level characteristics, and ε is the error term.

So far, I have proposed two different statistical methods as part of a natural experiment to determine whether there are significant differences between the means of the outcome variables for the CDH treatment and control groups. It was possible to exploit a natural experimental design given the characteristics presented by the CDH cash transfer programme and, also due to the availability of information, which includes pre- and post-

treatment data. However, the limitations of this methodology must be also considered, since two of the most important assumptions for the validity of this method are that treatment assignment is "as-if" random and that the outcome variables follow a normal distribution.

Particularly, when studying the effects of the CDH programme, there is a latent concern for a potential self-selection bias, arising from the fact that poor families are the ones who ultimately decide to switch programmes. The most certain is that this switching decision is based on individuals' own motivations and interests. If this were the case, CDH treatment assignment would not be entirely random, and therefore, the exogeneity condition would not be satisfied (i.e. endogeneity problems). As explained in the methodology chapter (see page 129), self-selection based endogeneity makes determination of causality more difficult, especially in research about the effects of programmes and policies (like the CDH cash transfers) on socio-economic outcomes.

Therefore, the results obtained in the natural experiment would be more reliable if we could control for all the factors that lead to self-selection among individuals. However, despite the number of controls included in my analysis, it is unlikely to take fully into account all the important characteristics influencing the selection process -mainly because some of the variables involved in such process are *unobserved*. Since I do not have information on key unobserved variables -such as the motivation of people when deciding whether to participate in the CDH programme and the skills required to implement a microenterprise- it is advantageous to implement alternative estimation methods (i.e. quasi-experimental designs), which are not based on a random selection of beneficiaries and can solve the self-selection problem applying statistical controls.

7.6.3 Instrumental Variable Method

One of the most common quasi-experimental designs is the instrumental variable (IV) estimation. This method is generally used to estimate causal effects in contexts in which

controlled experiments are not available and to address typical endogeneity problems in empirical quantitative analysis (Imbens & Angrist, 1994). However, if I want to correctly assess the causal relationship of an endogenous predictor (like CDH treatment) on any outcome under study, I need to find first a variable that fully satisfies the instrumental variable conditions of relevance and exogeneity (see page 134). In this thesis, I intend to use "previous credit experience" as a valid instrument of CDH treatment status. This variable is an indicator equal to 1 if, in the twelve months previous to the 2008 interview, any member of the household received cash loans or commercial credit, and 0 otherwise.

Specifically, I estimate different versions of the following two-stage regression model:

$$Y_i = \beta_1 W_i + \beta_2 CDH_i + \varepsilon_{zi} \,, \qquad (8)$$

$$CDH_i = \theta_1 W_i + \theta_2 CREDIT_i + \varepsilon_{xi} \,, \qquad (9)$$

Where Y is any of the outcome variables under study; *CDH* is a binary variable representing the treatment status (i.e. a dummy variable that equals 1 for poor households who took the decision to switch to the CDH programme); *CREDIT* is the instrumental variable of previous credit experience; W is a vector of individual- and household-level characteristics; $\beta's$ and $\theta's$ are parameters to be estimated; and ε_{zi} and ε_{xi} are random errors that are uncorrelated with each other and with their respective independent variables.

First, I estimate the second equation as a probit regression model (because the dependent variable is dichotomous) and, using the estimated equation, I calculate expected values for *CDH*:

$$E(CDH_i|W_i, CREDIT_i) = \widehat{CDH_i} = \hat{\theta}_1 W_i + \hat{\theta}_2 CREDIT_i$$

Then, in a second stage of the estimation, I replace CDH with $\widehat{CDH}$ in the first equation and estimate the parameters of interest by a method suited to the measurement of *Y* according to the type of dependent variable (for example, ordinary least squares if the dependent variable is continuous, ordered logistic if it is an ordinal discrete variable, probit if it is a binary, etc.).

7.6.4 Difference-in-Difference

The other quasi-experimental design that I use is the difference-in-difference method. Since I have information at two different periods of time, both for the CDH treatment and control groups, it is possible to isolate the externalities correcting for potential self-selection bias and endogeneity problems (associated with the CDH programme implementation). Thus, this method allows a more precise calculation of the effects of the CDH treatment among BDH beneficiaries.

As explained above (see Chapter 5, page 183), to use this method it is necessary to convert the panel type database from a wide format to a long format and create indicators of observation year (2008 and 2014) and group (treatment and control). In this case, the control group is made up of those beneficiaries of the BDH programme who have not received CDH cash transfers, while the treatment group are those who decided to switch programmes at some point. Formally, we would have a CDH treatment ($j = 1$) and a comparison ($j = 0$) group for both the before ($t = 0$) and after ($t = 1$) periods. Accordingly, the estimated regression model is the following:

$$Y_{it}^{j} = \beta_0 + \beta_1 TIME_t + \beta_2 CDH^{j} + \delta(TIME * CDH)_j^t + \beta_3 X_{it}^{j} + \varepsilon_{it}^{j} \qquad (10)$$

Where Y_{it}^{j} is the level of the outcome variable of a given individual i in group j at time t. CDH^{j} is a dummy variable that takes a value equal to 1, if the observed person belongs to the treatment group, and takes value equal to 0, if the person is in the control

group. $TIME_t$ is another dummy variable, which takes a value equal to 1 in the post-treatment period; and 0 otherwise. The diff-in-diff estimator is δ, the coefficient of the interaction between CDH^j and $TIME_t$. Note that this interaction term is also a dummy variable, which takes a value equal to 1, only for the CDH treatment group in the post-treatment period. Lastly, X_{it}^j is a set of control variables and ε_{it}^j is the error term.

Note that the sample used for the evaluation by this method is made up of the same individuals as by the previous methods, but with twice as many observations as we are going to compare them at two different time points. Now that the theory-based approach to the CDH evaluation has been carried out, the sample selection process has been explained and the different measurement models have been presented, it is time to describe the obtained results on the effects of the CDH programme on the different outcome variables.

CHAPTER 8

Measuring the Impact of the Crédito de Desarrollo Humano

8.1 Introduction

In this chapter, the results about the outcome effects of the Crédito de Desarrollo Humano (CDH) on well-being, human capital and unemployment are discussed using different impact evaluation methodologies. The chapter begins by analysing the post-treatment characteristics of the sample. This is followed by the results of the natural experimental method, which are obtained by two different econometric techniques. Finally, the results obtained by the diff-in-diff and IV methods are presented and discussed as two different possibilities to address the potential self-selection problems.

The results presented below will serve to better understand whether it is effective to set aside conditionality and trust those in poverty with lump-sum cash transfers. Empirical evidence can contribute to an understanding of whether the CDH cash transfers are as or more efficient than traditional conditional cash transfers (CCTs). In addition, these results could make a significant contribution to the future design of social assistance

programmes, the development of more precise targeting methodologies, and the understanding –on the part of programme administrators– of the importance of a systematic collection of longitudinal data to facilitate the quasi-experimental evaluation of the programmes.

8.2 Post-treatment Characteristics of the Sample

The end-line descriptive statistics by sample group are presented in Table 28. This information is useful to have an idea if there were changes in the outcome variables after 6 years of the baseline and, above all, if there are differences between the treatment and control groups in the period after the intervention. When comparing the pre- and post-treatment characteristics of the sample, it is clear that poor people presented a considerable improvement in their level of well-being and in most of the outcome variables. For instance, the mean of the whole sample's welfare index goes from 22.033 points in 2008 to 32.052 points in 2014. This means that, after 6 years, the level of well-being of the individual sample members increased by approximately 45 percent.

Table 28: Post-intervention Descriptive Statistics by Sample Group

	Control			CDH Treatment		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Welfare Index	31.284	11.128	385,371	32.821	10.595	334,616
Educational Attainment	2.501	1.216	385,371	2.693	1.182	334,616

Floor Material	4.443	1.254	385,371	4.556	1.180	334,616
School Enrolment	0.790	0.406	52,947	0.808	0.393	49,041
School Attendance	0.765	0.423	52,947	0.784	0.411	49,041
Unemployment	0.007	0.085	385,371	0.008	0.091	334,616

The results also show improvements over time in the households' educational attainment, the quality of the floor material and the first-born's probability of school enrolment. On the other hand, the probabilities of school assistance and unemployment appear to be slightly lower and higher respectively in the post-treatment period. These descriptive statistics are important for the quantitative analysis because they give us a general idea of trends and basic features of the data. They provide simple summaries about the sample and the outcome measures; however, they do *not* determine the factors that have influenced the change over time in the outcome variables. Therefore, in order to know if the CDH programme has had any effect on the level of welfare of the families, it will be necessary to use more advanced econometric techniques.

Moreover, it is also evident from Table 28 that in 2014, after the treatment group received the CDH transfers (at least once), the sample groups were no longer practically homogeneous because some significant differences are visible in the means of the outcome variables. In this case, the end-line descriptive statistics indicate that the treatment group is in a better welfare situation in relation to the control group. For example, the mean of the welfare index in the control group is 31.28 points, while the CDH beneficiaries present a mean welfare of 32.82 points. Therefore, without considering that beneficiaries self-select themselves to participate in the programme (i.e. assuming that the allocation of transfers would be random) and not taking into account possible omitted variables, these results at least suggest that the CDH had a positive effect on the level of well-being of those in poverty. Anyway, only through regression analysis can we be sure of the magnitude and statistical significance of the association between the CDH

programme and well-being, since only in this way the possible problems of endogeneity are addressed.

It is finally worth noting that in this case the control group is in a very slightly better position only in terms of the probability of unemployment. More specifically, the probability of being unemployed in the post-treatment period is 0.1 percentage points lower in the control group. This was not the case in the period before the CDH treatment implementation, when the two groups presented the exact same levels of unemployment. Nevertheless, it is not possible, at least for the moment, to attribute this small difference to the effect of the CDH transfers on labour supply. Once again, this will be statistically determined by the different quantitative evaluation methods presented below.

8.3 Natural Experimental Results

The natural experimental design starts with difference-of-means tests (two-sample t tests with equal variances) in order to confirm that after the intervention there are significant differences in the means of the outcome variables between both sample groups. The results obtained from a series of t-tests are summarized below:

Table 29: Difference-of-Means Tests (Control Group vs. CDH Treatment Group)

	Group									
	Control			Treatment (CDH)			t-test for Equality of Means			
	Mean	SD	n	Mean	SD	n	95% CI for Mean Difference	t	df	Sig. (2-tailed)
Welfare Index	31.284	11.128	385,371	32.821	10.595	334,616	-1.58, -1.48	-59.78	719,985	0.00
Educational Attainment	2.501	1.216	385,371	2.693	1.182	334,616	-0.19, -0.18	-67.57	719,985	0.00

Floor Material	4.443	1.254	385,371	4.556	1.180	334,616	-0.11, -0.10	-39.1	719,985	0.00
School Enrolment	0.790	0.406	52,947	0.808	0.393	49,041	-0.02, -0.01	-7.00	101,986	0.00
School Attendance	0.765	0.423	52,947	0.784	0.411	49,041	-0.02, -0.01	-7.27	101,986	0.00
Unemployment	0.007	0.085	385,371	0.008	0.091	334,616	-0.001, -0.0007	-5.41	719,985	0.00

* p-value < 0.05. Ho: diff = 0.

In all cases, the null hypothesis of equality of means can be rejected because, at a confidence level of 95 percent, the p-values of the t-tests are 0.00 (which does not surpass the significance level of 0.05). Thus, these results confirm that there are statistically significant mean differences between the control group and CDH treatment group in all the outcome variables. More specifically, they suggest that opting for CDH cash transfers has positive effects on the level of well-being, educational attainment, quality of floor material, school enrolment and school attendance. In fact, the only negative effects of this type of cash transfers seem to be on the probability of unemployment, although the magnitude of the impact (if any) should be very small.

In terms of the welfare index, CDH beneficiaries present a higher mean than those in the control group. According to the 95 percent confidence interval for the estimated mean difference, the CDH treatment increases individuals' scores by a maximum of 1.58 points. Therefore, this programme considerably improves the well-being situation of those in poverty, since an increase of almost two points of the SELBEN Welfare Index is not easy to achieve in such a short time, much less with a change that at least operationally is simple. In this case, it is basically achieved by delivering cash transfers in a different way (i.e. accumulated) and without conditions, contrary to what the paternalistic vision of poverty indicates.

Moreover, the results also indicate that switching programmes for at least one year increases the probability of school enrolment and attendance of the first-born in around 2 percentage points. This is despite the fact that there is no way to condition the delivery of transfers to beneficiaries sending their children to school. On the other hand, the CDH programme seems to increase the probability of unemployment in 0.1 percentage points. Overall, the t-tests confirm that people who received CDH cash transfers (one or more times) are in a better situation than those who chose to stay with the traditional programme for the whole time period. However, there may be omitted variables problems, thus, in order to determine the causal effects of the CDH, it is necessary to employ different regression models (i.e. OLS, probit and ordered logistic) as econometric methods. The estimations of regression analysis (assuming random selection of CDH beneficiaries) are presented below.

8.3.1 The Effects of the Crédito de Desarrollo Humano on the Welfare Index

The OLS regression estimates of the impact of the CDH cash transfers on the SELBEN Welfare Index are presented in Table 30. All the regressions in the table control for province of residence, marriage status, and self-defined ethnicity fixed-effects. In the first and most basic specification, with the mere inclusion of control dummies, there are sizeable positive effects of the CDH on the level of well-being. More specifically, the decision to switch to the CDH programme to receive the transfers accumulatively increases the beneficiaries' welfare index by about 1.5 points and the estimate is highly statistically significant.

The magnitude of the CDH coefficient is reduced when the household's average educational attainment is included as a regressor in column (2). Thus, part of the CDH effect was driven by higher education levels of those who opted for the new programme, which may also be due to the accumulated transfers. Note that the effects of the CDH

programme on the level of welfare can be direct, but also indirect through some of its components. These indirect effects will be further analysed in the next section of this chapter.

Something similar happens in column (3), when household's quality of floor material is included in the analysis. The CDH coefficient is still positive and significant, but it is once again reduced in magnitude. Apparently, another part of the effect of the CDH on welfare may be due to the direct effect of the alternative cash transfers on the household's floor material (a proxy of income level), which is also a component of the Welfare Index. This will also be confirmed in the next section.

The estimated coefficients and their statistical significance remain practically unchanged when the unemployment indicator is included in column (4), while their magnitudes somewhat increase when the age of individuals is included in the last specification (column 5). The latter seems to indicate that a small part of the effects of the CDH on the level of welfare were being offset by the age of individuals. Furthermore, given that the inclusion of the unemployment indicator does not alter the CDH coefficient, it seems that changing the programme does not affect (or only slightly affects) the probability of being unemployed. This will also be discussed in depth in Section 8.3.4.

Overall, the obtained results indicate that the CDH programme has a positive and significant effect on the level of well-being among the poor. As different control variables (including two components of well-being) are added to regressions, the magnitude of the CDH coefficient is gradually reduced until it stabilizes at 1.1 points. Moreover, and as in the Bono de Desarrollo Humano (BDH) impact evaluation, the independent effects of household's educational attainment, quality of floor material, unemployment and individual's age (a labour experience proxy) on welfare are positive and significant.

8.3.2 The Effects of the Crédito de Desarrollo Humano on Two Different Welfare Components

In order to determine if the CDH cash transfers also affect the level of well-being indirectly through some of its components, I estimate the independent effects of this programme on the educational attainment of the households and the quality of the floor material. As previously explained, both dependent variables are ordinal categorical that take on three or more values and, therefore, their analysis requires ordered logistic regression models. These models predict the probability of an individual appearing in each successively higher category of the outcome variable.

The results summarized in Table 31 indicate that the effect of the CDH programme on the households' educational attainment is positive and statistically significant. In the first specification (column 1), which includes only few control dummies, the ordered log-odds (logit) coefficient on the CDH indicator is 0.2631. Recall that this estimate results from the comparison between those affected by the CDH and those not affected in terms of expected educational attainment. Thus, the ordered log-odds for CDH-affected individuals of being in a higher education category are 0.2631 more than for individuals not affected, when the other variables are held constant. This is perhaps easier to understand when expressed in terms of simple probability (see Appendix D), according to which, if an individual receives CDH transfers, the probability (p) that his household is at a higher category of education is 0.5654. While, on the other hand, if an individual only receives the BDH transfers, the probability (p) that his household is at a higher category of education is 0.4346 [p (BDH) = (1-p (CDH)].

In terms of proportional odds ratios (second half of column 1), the interpretation of the CDH coefficient would be that for individuals affected by this programme the odds of being in a higher education category are 1.3010 times the odds for those not-affected individuals, given that the other variables are held constant in the model. Thus, the chances

of being more educated are greater for people who receive CDH cash transfers. This proportional odds ratio is calculated with the following formula:

$$1.3010 \text{ (proportional odds ratio)} = \frac{0.5654\ (p \text{ of being in a higher education category for CDH affected individuals})}{0.4346\ (p \text{ of being in a higher education category for those not affected by the CDH})}$$

The impact of the CDH programme is still positive and significant when more control variables are progressively included in the regressions (column 2 to 4). In fact, the ordered log-odds estimate and the proportional odds ratio remain practically unchanged when floor material –another component of well-being– is controlled in column (2) and when the unemployment indicator is included in column 3. These results suggest that the CDH effects on educational attainment are direct and not through the proxy of income level. Moreover, the estimated impact of the CDH programme is not driven by possible differences in the probability of unemployment between beneficiaries and non-beneficiaries of the cash transfers.

Similarly, the results reported in Table 32 show a significant positive effect of the CDH programme on the household's quality of floor material. In the most basic specification, the estimated ordered logit coefficient on the CDH indicator is of 0.1977 log-odds units. Therefore, if an individual receives CDH cash transfers, the probability (p) that his household is at a higher floor material category is 0.5493. Evidently, this probability is greater than that of individuals who receive only BDH transfers (i.e. 0.4507). By relating these two probabilities, the proportional odds ratio coefficient is calculated. One knows that the effect of the CDH is positive since this coefficient is greater than 1. The specific interpretation is that the probability of being in a higher category of floor material for a person affected by the CDH is 1.2185 times the probability for an unaffected person, given that the other variables remain constant in the model.

The estimated coefficients are somewhat reduced, but not significantly, when more control variables are added. By including educational attainment as a regressor, the most

considerable reduction of the CDH coefficient occurs (i.e. 0.1586 log-odds units). Apparently, a small part of the effect on the proxy of the income level occurs through educational attainment. These results are consistent with those previously calculated and, in addition, with the notion that higher levels of education lead to better levels of income and well-being. On the other hand, the effect of the CDH remains almost constant when the unemployment indicator and the age of the individuals are included, indicating that the results are not driven by differences in the probability of unemployment and labour experience.

Overall, the results presented in Tables 31 and 32 show that the CDH programme effects on welfare are not only direct, but also indirect. Its impact on the level of welfare occurs through its main components, improving the educational achievement and quality of life of poor families. One possible explanation for these findings is that CDH recipients have more incentives to study and improve their skills. It seems quite likely that those in poverty are aware that in order to get the most out of their productive investments, it might be essential to acquire knowledge through formal education.

8.3.3 The Effects of the Crédito de Desarrollo Humano on Human Capital of the First-born Children

Apart from the positive effects of the CDH programme on the general welfare of poor families, it is possible that –unlike the BDH and in spite of the absence of conditionalities– cumulative cash transfers do have an impact on the investment decisions in the human capital of the children. Given that according to the results estimated above, there is an impact on the average educational attainment of the households, it remains to be known which family members are those who decided to improve their educational level as a result of the money available for productive investments.

Specifically, in this thesis I focus on the effects on school enrolment and school attendance of first-born children, who, as explained in Chapter 4, are the most exposed to drop-out from school in order to work with their parents (see pages 178 and 179 for a detailed justification). One might think that the greater availability of monetary resources should encourage parents to send their children to school, but it is also likely that if they decide to invest a considerable amount of money (and time) in their business, they would need the help of other family members to reduce the inherent risks and increase the chances of success. That is why it is necessary to determine with certainty if the CDH programme affects this decision in any way.

Table 33 presents the CDH effects on the probabilities that the first-born children are enrolled in school (first half) and attending school (second half). Recall that due to the binary nature of the outcome variables (i.e. dichotomous variables), I use probit regression models in the analysis, which predict the likelihood of an event to occur on the basis of the included regressors. They do so by using the cumulative distribution function of the standard normal. Note that for this analysis, a smaller sample is employed, since not all beneficiary families have children.

In column (1), the outcome variables are regressed only on the CDH indicator and the set of basic control dummies. This probit regression results show significant positive effects of the programme on first-born children school enrolment and attendance. Actually, both estimated CDH coefficients are very similar in magnitude and they suggest that those first-born affected by the CDH programme are about 2 percentage points more likely to be enrolled and attending school (holding all other explanatory variables constant at zero). More specifically, the predicted probabilities are the following:

$$Probability\ of\ enrollment = F(constant + \ CDH\ coef. * CDH\ indicator)$$

$$F(0.4702 + 0.0430) = 0.6960 \ \ (affected\ by\ the\ CDH)$$

$$F(0.4702) = 0.6808\ (not\ affected\ by\ the\ CDH)$$

$$Probability\ of\ attendace = F(constant + \ CDH\ coef. * CDH\ indicator)$$

$$F(0.3644 + 0.0405) = 0.6572 \ \ (affected\ by\ the\ CDH)$$
$$F(0.3644) = 0.6422\ (not\ affected\ by\ the\ CDH)$$

where F is the Cumulative Distribution Function (CDF) of the standard normal.

However, when more regressors are included progressively in the specifications (columns 2 to 5), the coefficients on the CDH indicator become statistically insignificant. These results indicate that all the positive effect on the probabilities of school enrolment and attendance was driven by differences in educational and income levels of parents, their employment status and age. Therefore, there are other factors that affect both positively and negatively the decision to send the first-born children to school, but not the CDH programme.

So far, both cash transfer programmes evaluated in this thesis (the one with and the other without conditions) seem little or no effective in meeting the goal of promoting investment in children's human capital – at least with respect to the eldest child, since it is still possible that there are effects on the other children (i.e. younger siblings). In fact, this was precisely the answer they gave me as a possible explanation when I briefly comment on the results of this analysis to the Ministerio Coordinador de Desarrollo Social (MCDS) officials. Of course, it is understandable that they support this argument, since it is important for them to promote and maintain the idea that both social assistance programmes impact somehow the school attendance of the children. This is the easiest way to justify and legitimize their implementation before public opinion.

8.3.4 The Effects of the Crédito de Desarrollo Humano on Unemployment

In order to determine if the CDH programme has created employment disincentives, and if the absence of work-related conditionalities has affected the labour market engagement

of the beneficiary households, I estimate the effects of the CDH on the probability of unemployment. The probit regression results are presented in Table 34.

In the first specification, the estimated CDH coefficient is positive and statistically significant (even at the 1 percent level). Although the magnitude of the effect is very small (i.e. less than one percentage point). Holding all control dummies constant at zero, the predicted probabilities of being unemployed –for those individuals affected and not affected by the CDH cash transfers– are calculated as follows:

$$Probability\ of\ unemployment = F(constant + \ CDH\ coef. * CDH\ indicator)$$
$$F(-3.1748 + 0.0524) = 0.0008\ \ (affected\ by\ the\ CDH)$$
$$F(-3.1748) = 0.0007\ (not\ affected\ by\ the\ CDH)$$

where F is the cumulative distribution function of the standard normal. Therefore, according to these results, individuals affected by the CDH are only 0.01 percentage points more likely to be unemployed.

The estimated positive impacts of the CDH are very similar and also little considerable –despite being statistically significant– when more control variables are added to the probit regression. In the second and third specifications, the coefficients on the CDH indicator show once again that those individuals affected by this type of cash transfers are about 0.01 percentage points more likely to be unemployed (holding educational attainment, floor material, and all other explanatory variables constant at zero). The effects are calculated by $[F(-3.3831 + 0.0385) - F(-3.3831)]$ for column (2) and by $[F(-3.451 + 0.0377) - F(-3.451)]$ for column (3).

Finally, in the full specification (column 4), the positive effect of the CDH programme remains statistically significant and its magnitude is a little more considerable. Once the age of individuals is controlled, those affected by this programme are about 0.02 percentage points more likely to be unemployed (holding all other explanatory variables constant at zero) $[F(-2.899 + 0.0210) - F(-2.899)]$. The slight increase in the magnitude of the effect may suggest that part of the CDH impact was being cancelled by

age differences among individuals in the treatment and control groups. Besides, the inclusion of age as a regressor guarantees that the positive impact of the CDH on unemployment is not being driven by the labour experience (approximated by age) of cash transfers-affected individuals.

In general, the unemployment regression results indicate that the CDH unconditional cash transfers programme did not create strong disincentives to work among the beneficiaries. Although the programme slightly increases the probability of being unemployed, this increase is not significant. The evidence, in this case, does not show that the recipients of the accumulated transfer have reduced their intensity of job search or their willingness to accept a job. Therefore, these results contrast with the paternalistic and welfare trap approaches, according to which these programmes create perverse incentives not to work, and much more when there are no conditionalities. It seems that, at least in Ecuador, the conditions related to work are not necessary to indicate that the CDH programme is effective reducing poverty without affecting the labour participation of the beneficiaries.

8.3.5 Lessons from the Natural Experimental Results

Overall, the natural experimental results suggest that the CDH programme has had a positive effect, both directly and indirectly, on the level of welfare among the poor. The indirect effects occur through some of the most important components of well-being, such as the educational attainment of the household and the level of income, which are positively affected when the BDH beneficiaries opt instead for this alternative programme. In addition, no evidence was found that the CDH has a negative impact on the probability of school enrolment and school attendance of the first-born. On the other hand, a very small but statistically significant positive effect on the probability of unemployment was

estimated, indicating that receiving the cash transfers in a single lump sum payment could generate some disincentive to work.[88]

These results imply that the efficiency levels presented by the CDH programme are, to a large extent, aligned with the theories of change described in the previous chapter (see page 270), that is, the observed results are similar to what is expected to be obtained of the programme or to what in theory it should offer. In terms of well-being, when comparing the estimated effects with the expected results, we can see that the programme improves the quality of life, reduces poverty and increases the economic capacity of the beneficiary households. On the other hand, in terms of the inter-temporal transmission of poverty, the non-conditional cash transfers are not enough to promote school enrolment and attendance of the first-born children. However, the CDH partially fulfils the objective of facilitating access to knowledge, since the average educational attainment of the households is affected in a substantial and positive way by the programme. Finally, in terms of significantly reducing unemployment and underemployment, it seems that the results of the CDH are scarce or even slightly counterproductive. It could be said that the programme does not meet the expected results in terms of unemployment, but it will be necessary to confirm this and the other findings through the subsequent use of more adequate quantitative methods. In general, it seems quite likely that both the type of intervention and the dynamics of the CDH programme are sufficient to achieve at least the majority of the desired results. Nevertheless, it is necessary to recognize that perhaps some aspects of the intervention thoroughly described by the theories of change (resources/inputs, activities, mechanisms or channels of change) must be corrected or adjusted so that the CDH functions more efficiently in the near future.

So far, the estimated results clearly indicate that the beneficiaries of the CDH programme have experienced much better outcomes in terms of welfare and poverty reduction than those who received the traditional cash transfers. Thus, the element of

[88] The last finding will be discarded when the problems of endogeneity are solved using alternative quasi-experimental methods.

conditionality in social assistance practices is once again challenged by the obtained results. While the BDH evaluations suggest that conditions alone do not guarantee the success of a programme, the natural experimental evaluation of the CDH shows that the expected results can be achieved even with (and perhaps due to) the absence of conditions. In other words, these findings are opposed to the widespread idea that conditionality is a necessary (and even sufficient) element for cash transfers to effectively reduce poverty and improve the quality of life of families. On the contrary, at least in the Ecuadorian case, higher levels of efficiency are achieved with an unconditional programme.

Nevertheless, as mentioned above, the validity of these results is threatened in some way by the possible presence of endogeneity problems. One of the features of the CDH programme is that poor families self-select themselves to receive the accumulated transfers, so the treatment group could have more things in common than just participating in this initiative. Actually, there might be other characteristics that explain their decision to opt for this programme. Thus, it is possible that these unobserved characteristics are those that actually have the positive effect on well-being. For that reason, it becomes imperative, as part of the evaluation of the CDH , to complement the analysis with alternative quasi-experimental methods that address the potential problems of omitted variables and reverse causality. In the following section, I present difference-in-differences results using a sample composed of the same individuals, but at two different points of time (i.e. before and after the implementation of the CDH).

Table 30: Regression Estimates of the Effect of the CDH on the Welfare Index

(Standard Errors in Parentheses)

Independent variable	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)
CDH Indicator	1.4548*** (0.0237)	1.0576*** (0.0227)	0.7967*** (0.0199)	0.7967*** (0.0199)	1.0717*** (0.0197)
Household's Educational Attainment	---	2.4883*** (0.0095)	1.9286*** (0.0084)	1.9285*** (0.0084)	2.2526*** (0.0086)
Household's Floor Material	---	---	3.8819*** (0.0083)	3.8819*** (0.0083)	3.9242*** (0.0081)
Unemployment Indicator	---	---	---	0.0306 (0.1112)	0.4512*** (0.1096)
Age	---	---	---	---	0.1390*** (0.0009)
Constant	21.6176*** (0.0785)	17.2098*** (0.0770)	3.1772*** (0.0738)	3.1775*** (0.0738)	-2.8563*** (0.0834)
Reported Province of Residence Dummies	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓
Number of Observations	716,497	716,497	716,497	716,497	716,497
R-squared	0.1781	0.2486	0.4238	0.4238	0.4408

Note: Estimates are from linear regression models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)

Table 31: Household's Educational Attainment Regressions (CDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
	Dependent Variable: Household's Average Educational Attainment							
CDH Indicator	0.2631***	1.3010***	0.2461***	1.2790***	0.2456***	1.2784***	0.1521***	1.1643***
	(0.0044)	(0.0057)	(0.0044)	(0.0056)	(0.0044)	(0.0056)	(0.0044)	(0.0052)
Household's Floor Material	---	---	0.2213***	1.2476***	0.2210***	1.2474***	0.2019***	1.2237***
			(0.0018)	(0.0023)	(0.0018)	(0.0023)	(0.0018)	(0.0022)
Unemployment Indicator	---	---	---	---	0.6519***	1.9192***	0.4898***	1.6320***
					(0.0253)	(0.0486)	(0.0254)	(0.0416)
Age	---	---	---	---	---	---	-0.0469***	0.9541***
							(0.0002)	(0.0002)
Number of Observations	716,497	716,497	716,497	716,497	716,497	716,497	716,497	716,497
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 32: Household's Quality of Floor Material Regressions (CDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
Dependent Variable: Household's Quality of Floor Material								
CDH Indicator	0.1977***	1.2185***	0.1586***	1.1719***	0.1585***	1.1718***	0.1445***	1.1555***
	(0.0045)	(0.0055)	(0.0045)	(0.0053)	(0.0045)	(0.0053)	(0.0045)	(0.0053)
Household's Educational Attainment	---	---	0.2466***	1.2797***	0.2464***	1.2794***	0.2287***	1.2570***
			(0.0019)	(0.0025)	(0.0019)	(0.0025)	(0.0020)	(0.0025)
Unemployment Indicator	---	---	---	---	0.0953***	1.1***	0.0723***	1.0750***
					(0.0260)	(0.0286)	(0.0260)	(0.0279)
Age	---	---	---	---	---	---	-0.0073***	0.9926***
							(0.0002)	(0.0002)
Number of Observations	716,497	716,497	716,497	716,497	716,497	716,497	716,497	716,497
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 33: Regression Estimates of the Effects of the CDH on School Enrolment and School Attendance of the First-born Child

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)	Probit Regression (5)
Dependent Variable: First-born Child Enrolled in School Dummy					
CDH Indicator	0.0430*** (0.0091)	-0.0030 (0.0095)	-0.0052 (0.0095)	-0.0051 (0.0095)	-0.0088 (0.0095)
Household's Educational Attainment	---	0.4510*** (0.0054)	0.4460*** (0.0055)	0.4462*** (0.0055)	0.4436*** (0.0055)
Household's Floor Material	---	---	0.0342*** (0.0038)	0.0342*** (0.0038)	0.0336*** (0.0038)
Unemployment Indicator	---	---	---	-0.1154* (0.0629)	-0.1190* (0.0629)
Age	---	---	---	---	-0.002*** (0.0006)
Constant	0.4702*** (0.0267)	-0.5153*** (0.0300)	-0.6361*** (0.0330)	-0.6369*** (0.0330)	-0.535*** (0.0454)
Number of Observations	101,586	101,586	101,586	101,586	101,586
Dependent Variable: First-born Child Attending School Dummy					
CDH Indicator	0.0405*** (0.0089)	-0.0021 (0.0092)	-0.0049 (0.0092)	-0.0048 (0.0092)	-0.0092 (0.0092)
Household's Educational Attainment	---	0.4221*** (0.0052)	0.4159*** (0.0053)	0.4160*** (0.0053)	0.4130*** (0.0053)
Household's Floor Material	---	---	0.0425*** (0.0037)	0.0426*** (0.0037)	0.0418*** (0.0037)
Unemployment Indicator	---	---	---	-0.0905 (0.0616)	-0.0950 (0.0616)
Age	---	---	---	---	-0.002*** (0.0006)
Constant	0.3644*** (0.0260)	-0.5675*** (0.0291)	-0.7180*** (0.0320)	-0.7186*** (0.0320)	-0.594*** (0.0441)
Number of Observations	101,586	101,586	101,586	101,586	101,586
Province of Residence Dummies	✓	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.

*** Significant at the 1 percent level (P value<0.01)

* Significant at the 10 percent level (P value<=0.1)

Table 34: Unemployment Regressions (CDH)

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)
Dependent Variable: Dummy for Being Unemployed				
CDH Indicator	0.0524***	0.0385***	0.0377***	0.0210**
	(0.0103)	(0.0104)	(0.0104)	(0.0105)
Household's Educational Attainment	---	0.1081***	0.1060***	0.0844***
		(0.0038)	(0.0039)	(0.0040)
Household's Floor Material	---	---	0.0177***	0.0144***
			(0.0045)	(0.0045)
Age	---	---	---	-0.014***
				(0.0005)
Constant	-3.1748***	-3.3831***	-3.451***	-2.899***
	(0.0449)	(0.0460)	(0.0493)	(0.0541)
Number of Observations	716,497	716,497	716,497	716,497
Province of Residence Dummies	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.

*** Significant at the 1 percent level (P value<0.01)

** Significant at the 5 percent level (P value<0.05)

8.4 Difference-in-Difference Results

As explained in Chapter 3, the self-selection of beneficiaries can become a problem for the estimation and inference of the results if the decision to request the CDH is correlated with the outcome variables. Therefore, it is necessary to address this potential endogeneity issue by proposing alternative methods of evaluation. In this section, I present the results of one of the simplest but most powerful econometric techniques to estimate the effects of the CDH treatment while addressing self-selection bias: the "difference-in-differences" estimator. The models presented below include different specifications with relevant independent variables that could vary over time (i.e. educational attainment, housing conditions, employment situation, etc.) for the estimation to be more reliable.

8.4.1 The Diff-in-Diff Effects of the Crédito de Desarrollo Humano on the Welfare Index

The diff-in-diff model used to estimate the effects on the Welfare Index employs the ordinary least squares estimator. The model is very flexible and reasonably easy to interpret. Moreover, it allows for the inclusion of additional regressors that are thought to influence the amount of change predicted by the CDH treatment. Different variables that vary over time such as household's educational attainment, the quality of floor material, the unemployment indicator and individual's age are progressively included to minimize the possibility that self-selection biases the obtained results. The difference-in-differences estimates are presented in Table 35.

In the first specification, the interaction coefficient (CDH x Post-treatment period) confirms that there is a positive effect of the CDH programme on the level of welfare. The magnitude of the effect is even greater than that estimated by the previous method, which does not take into account the selection problems. More specifically, the diff-in-diff results in column (1) show that the accumulated cash transfers directly increase the Welfare Index

by about 1.56 points. In addition, as demanded by this econometric technique, both time and group indicators are included as independent explanatory variables. This is done to account for the specific characteristics of each group and the trends over time that are likely to affect the outcome variable and skew the impact of the cash transfers.

The 2014-indicator coefficient is positive and statistically significant, indicating that the trend over time is for all individuals to improve their welfare situation considerably. In the post-treatment period, individuals improve their level of well-being by about 9 index points due to unmeasured factors that vary over time. Remember that this trend is the same for both groups (i.e. treatment and comparison). On the other hand, the treatment group indicator coefficient is negative and significant, indicating that the fixed traits of the individuals that receive the CDH cash transfers actually negatively affect their level of well-being. However, the magnitude of the effect of the treatment group intrinsic characteristics on the welfare level is small, reflecting that these unobserved variables –correlated with the decision to opt for CDH transfers– were not biasing the estimated impact of the programme significantly. Specifically, the welfare index is reduced by just 0.05 points due to the specific characteristics of those who prefer to receive the accumulated transfers.

The results do not vary much when adding regressors that vary over time in order to minimize the possibility of the assumption of common trends being violated. In column (2), when household's average educational attainment is included, the CDH estimate is still positive and its magnitude is hardly altered. The impact of the CDH programme is only slightly reduced by the positive effect that each completed educational degree has on the level of well-being (2.25 index points). Moreover, the coefficients on the 2014-indicator and the treatment group indicator are still positive and negative respectively. Although their magnitudes change a bit, they still strongly suggest that the trends over time and the group specific characteristics affect the level of well-being. Therefore, the presence of the household's average educational attainment does not change the facts that the welfare index increases considerably in the post-treatment period for all the individuals

and that the fixed traits in the treatment group slightly (and negatively) affects the level of well-being.

Something similar occurs in columns (3), (4) and (5) when the quality of the floor material, the unemployment indicator and the age of the individuals are progressively added to the model. The magnitude of the CDH coefficient is reduced a little in each specification, but maintains its positive sign and its statistical significance. In fact, in the full specification all the explanatory variables are significant, the model has a considerable predictive power (R-squared) and the estimated effect of the CDH programme is 1.21 index points. This indicates that there was indeed a small selection bias and, once it is addressed, the impact of the CDH is somewhat greater (around 0.14 index points more than the impact estimated with the previous method).

Additionally, according to these specifications, the trend over time is for the welfare index to increase by around 7 points in the post-treatment period for both groups. While the specific (fixed) characteristics of the treatment group maintain their small negative effect (between 0.30 and 0.11 index points) once more control variables are included. Finally, as expected, the effects of educational attainment, floor material (proxy of income level), unemployment situation and age on the Welfare Index are positive, as indicated also by the previous econometric method. The proxy of the income is the one that has the greatest impact on welfare, increasing the index by 3.23 points each time the quality of the floor material rises one category. On the other hand, the impact of each additional year of age is only 0.08 index points.

8.4.2 The Diff-in-Diff Effects of the Crédito de Desarrollo Humano on Two Different Welfare Components

In this section, I intend to confirm the indirect effect of the CDH programme on the level of welfare through two of its most important components. The diff-in-diff models used to

estimate the CDH effects on households' average educational attainment and quality of floor material employ the ordered logistic estimator. Tables 36 and 37 summarize the diff-in-diff regression results in terms of ordered log-odds (logits) and proportional odds.

The results presented in Table 36 corroborate that the CDH programme has a small but positive effect on the average educational attainment of the households. According to the first specification, the ordered log-odds for CDH-affected individuals of being in a higher education category are 0.0706 more than for people affected by the BDH programme. In terms of proportional odds ratios, the coefficient indicates that for CDH-affected individuals the odds of being in a higher category of education are 1.0732 times the odds for those BDH-affected individuals. Therefore, by interpreting these coefficients in terms of simple probability, it is slightly more likely that an individual's household is in a higher education category if he receives CDH transfers ($p = 0.5177$) than if he decides to remain enrolled with the other programme ($p = 0.4823$).

The ordered log-odds coefficients and the proportional odds ratios of the CDH indicator do not vary much when more control variables are included in the model. The same is true for the treatment group indicator log-odds coefficient, which is positive and statistically significant in all the specifications. In the full regression (column 4), it suggests that if an individual belongs to the treatment group, the probability that his household is in a higher category of education ($p = 0.5301$) is higher than the probability if the individual belongs to the comparison group ($p = 0.4699$). Thus, the fixed characteristics in the treatment group positively affect the average educational attainment of their households. On the other hand, the magnitude and the sign of the 2014-indicator coefficient vary from one specification to another. The most important change occurs when the age of the individuals is added to the model (column 4). In the end, the trend over time is for households to improve their average education levels, but this positive effect was being offset by the negative effect of age. The estimated ordered log-odds coefficient is 0.1890, indicating that it is more likely for a given household to be at a higher education category if the observation belongs to the post-treatment period ($p = 0.5471$).

Similarly, the results reported in Table 37 confirm that the CDH programme also positively affects the household's quality of floor material. In the most basic specification, the estimated ordered logit coefficient on the CDH indicator is 0.1823 log-odds units and thus its proportional odds ratio is 1.20. The interpretation, in terms of simple probabilities, would be that the odds of being in a higher floor material category for CDH-affected individuals (p = 0.5455) are higher than for those affected only by the BDH (p = 0.4545). Note also that the inclusion of more control variables does not significantly influence the magnitude of the CDH log-odds coefficient. In the second specification, with the inclusion of educational attainment, the CDH coefficient is reduced a little, but from there it remains almost constant.

The effect of the 2014-indicator is positive and significant in all the specifications. In fact, the magnitude of the logit coefficient is consistently large (around 0.80 log-odds units), indicating that the probability of being in a higher floor material category is higher in the post-treatment period (p = 0.69) than in the pre-treatment period (p = 0.31). Therefore, the trend over time was that poor households improved their housing conditions (and income level) considerably. Recall that this effect is due to unobserved variables that vary over time and has nothing to do with CDH cash transfers. On the other hand, in this case, the estimated ordered logit coefficient on the treatment-group indicator changes noticeably with the inclusion of more control variables. Although the effect of the fixed characteristics is always statistically significant, in any specification the magnitude of the effect is important. Specifically, the odds of being in a higher floor material category is almost equal for individuals in the treatment and comparison groups.

8.4.3 The Diff-in-Diff Effects of the Crédito de Desarrollo Humano on Human Capital of the First-born Children

The results found with the previous method (i.e. natural experiment) indicate that, despite the absence of conditionalities related to human capital, the CDH programme has no effect

(positive or negative) on the probability of enrolling and sending the first-born children to school. However, given the problems of self-selection, it is necessary to corroborate these results through a more advanced econometric strategy. I estimate difference-in-differences regression models of the impact of the CDH programme on two different binary dependent variables by using the probit estimator. As a result, these models calculate the predicted probability of children being enrolled and attending school on the basis of the regressors used in each specification.

The obtained diff-in-diff regression results are summarized in Table 38. The first half of the table presents the effects on the probability that the first-born child is matriculated in school, while the second half on the probability that he/she is attending classes. Note that the results for both outcome variables are very similar. In general, it is estimated that the CDH programme actually has statistically significant *negative* impacts, although the magnitudes of the effects in all the specifications are quite small. More specifically, it is a bit less likely for the first-born of a given individual to be enrolled and attending school if the household is affected by the CDH cash transfers. Taking as reference the last specifications and holding the values of all the other predictors constant at zero, the predicted probabilities of the first-born enrolling and attending school for individuals affected and not affected by the CDH are as follows:

$$Probability\ of\ enrollment = F(constant + \ CDH\ coef. * CDH\ indicator)$$
$$F(-0.088 - \ 0.082) = 0.4325 \ \ (affected\ by\ CDH)$$
$$F(-0.088) = 0.4649\ (affected\ by\ BDH)$$

$$Probability\ of\ attendance = F(constant + \ CDH\ coef. * CDH\ indicator)$$
$$F(-0.087 - 0.080) = 0.4336 \ \ (affected\ by\ CDH)$$
$$F(-0.087) = 0.4653\ (affected\ by\ BDH)$$

where F is the CDF of the standard normal. Therefore, the programme of unconditional and accumulated cash transfers reduces both the probability of being enrolled in school and attending classes in around 3 percentage points.

The estimated coefficients on the 2014-indicator are also negative and significant in all the specifications for both outcomes variables, indicating that it is less likely for the first-born of a given individual to be enrolled and attending school during the post-treatment period. In the full specifications (Column 5), the predicted probabilities of the first-born enrolling and attending school for observations in the pre- and post-treatment periods are as follows:

$$Probability\ of\ enrollment = F(constant + \ 2014\ coef. * 2014\ indicator)$$
$$F(-0.088 - 0.092) = 0.4285\ \ (post\text{–}\ treatment\ period)$$
$$F(-0.088) = 0.4649\ (pre\text{–}\ treatment\ period)$$

$$Probability\ of\ attendance = F(constant + \ 2014\ coef. * 2014\ indicator)$$
$$F(-0.087 - 0.161) = 0.4020\ \ (post\text{–}\ treatment\ period)$$
$$F(-0.087) = 0.4653\ (pre\text{–}\ treatment\ period)$$

Thus, according to these results, first-born children in the post-treatment period are about 4 percentage points less likely to be enrolled in school and 6 percentage points less likely to be attending school (holding everything else constant). As also indicated by the diff-in-diff results of the BDH programme, the trend over time is for households in a situation of poverty to worsen their behaviour towards their children's human capital. This is regardless of whether they are beneficiaries or not of social assistance programmes. The results of both evaluations (i.e. BDH and CDH) are consistent in this regard.

On the other hand, the impact of the treatment group indicator on both dependent variables is positive and statistically significant. Although the magnitudes of the estimated effects are small and they are reduced a little in each specification –due to the inclusion of more control variables–, all the probit coefficients suggest that the fixed characteristics

of the treatment group make it slightly more likely that their first-born children are enrolled and attending school. For example, in the last specifications, the predicted probabilities of the first-born enrolling in school will be of 47.54 percent $[F(-0.0616)]$ for individuals in the treatment group and 46.49 percent $[F(-0.088)]$ for individuals in the control group; while the predicted probabilities of the first-born attending school will be of 47.55 percent $[F(-0.0614)]$ for individuals in the treatment group and 46.53 percent $[F(-0.087)]$ for individuals in the control group. Therefore, first-born children in the treatment group are about 1 percentage point more likely to be enrolled and attending school due to the intrinsic characteristics of the individuals belonging to each group (holding everything else constant).

8.4.4 The Diff-in-Diff Effects of the Crédito de Desarrollo Humano on Unemployment

The diff-in-diff regression results for the effects of the CDH programme on the probability of unemployment are presented in Table 39. The models used in the analysis to estimate the predicted likelihood of being unemployed employ the probit estimator. In all the specifications, the CDH coefficients are statistically insignificant, indicating that having been affected by the unconditional cash transfers does not affect the predicted probability of being unemployed.

Therefore, despite the arguments of those who think that social assistance programmes without work-related conditions generate disincentives in the supply of labour, at least in this case, the evidence suggests that when an individual decides to change the programme opting for the accumulated transfers, the probability of being unemployed does not change. Taking into account the previously estimated results for the BDH (see page 253), according to which the conditional cash transfers positively impact labour supply, it is possible to infer that the CDH transfers would have the same positive

(although very small) effect if the comparison were with the individuals in poverty situation not affected by any of the programmes.

Moreover, the coefficients on the 2014-indicator are statistically insignificant in most of the specifications. It is only in the last one (Column 4) –with the presence of all the control variables– that the estimated probit coefficient is positive and significant (although small in magnitude), indicating that the trend in time is that the probability of being unemployed somewhat increases in the post-treatment period.[89] More specifically, holding everything else constant, individuals are about 0.03 percentage points more likely to be unemployed in the post-treatment period $[F(-3.0118 + 0.0635) - F(-3.0118)]$.

The opposite occurs with the coefficients on the indicator of the treatment group. In this case, the coefficients are positive and statistically significant in most specifications, with the exception of the last one (Column 4). In any case, the effect of the fixed characteristics of the group on the probability of unemployment is so small that, once expressed in terms of simple probability, it does not exceed 0.01 percentage points. These results indicate that the intrinsic features of the treatment group have little or no influence on their labour supply.

8.4.5 Lessons from the Difference-in-Differences Results

In general, the results obtained by the difference-in-differences method are consistent with those found by the natural experiment. Once the possibility of self-selection bias has been addressed, the statistical significance and the direction of the effects of the CDH on most of the outcome variables remain unchanged. However, the estimated effects specifically on unemployment and investment in human capital of the first-born change considerably with this more advanced econometric technique. The most important diff-in-diff results are summarized below and they are compared with those obtained previously. This is done

[89] Recall that this same trend over time in relation to the probability of being unemployed is estimated in the evaluation of the BDH (Chapter 6, page 254).

to understand the importance of using different quantitative methods in order to obtain more reliable estimates that allow reaching the correct conclusions.

The diff-in-diff regression results confirm that the CDH programme has a positive effect on the level of welfare of the poor. People who opted for the CDH at some point have on average 1.4 index points more in the SELBEN Welfare Index due specifically to the programme. Note that the effects on the Welfare Index estimated by both quantitative methods are quite similar. According to the natural experiment, the CDH programme increases welfare by about 1.2 index points, while according to the diff-in-diff method the impact is slightly higher.

Something similar happens with the estimated effects on the two welfare components that were analysed individually: households' average educational attainment and quality of floor material. The statistical significance and the positive direction of the CDH effects are the same with both quantitative methods. All the estimates reflect that when an individual receives the CDH cash transfers, the likelihood increases that their family will be more educated and their living conditions will be better. However, the magnitudes of the CDH ordered logit coefficients are considerably smaller when using the diff-in-diff approach. Apparently, self-selection problems were slightly biasing the initial estimation. Therefore, although the results are similar, it was necessary to take into account both trends over time and group specific characteristics –by using a more advanced evaluation technique– in order to obtain more accurate estimators of the effects of the CDH cash transfers.

The need to use more than one quantitative method, when there is only observational data to evaluate the effects of the CDH programme, is even more evident in the case of the outcome variables related to the investment in human capital of the first-born. The results of the differences-in-differences method reveal that the CDH programme of unconditional and accumulated cash transfers reduces the probability of the first-born being enrolled and attending school in around 3 percentage points. These findings contrast with those estimated using the simplest econometric method proposed in this thesis that

assumes a random allocation of transfers (i.e. natural experiment). According to this method, the effects of the CDH are also negative, but statistically insignificant; thus, indicating that the unconditional programme does not affect the human capital investment practices of those in poverty. The differences between the results found by both methods are very likely due to the self-selection of beneficiaries. The initially estimated absence of CDH effects was probably due to structural differences between the groups, which are later taken into account by the diff-in-diff method in order to reduce the possibility of biases.

Similarly, self-selection problems are somewhat more evident when estimating the effects of the CDH on unemployment. The diff-in-diff results indicate that having received unconditional cash transfers does not affect at all the predicted probability of being unemployed. While, according to the natural experiment, individuals affected by the CDH are 0.01 percentage points more likely to be unemployed. Although the magnitude of this effect is very small (i.e. less than one percentage point), it is statistically significant. Therefore, the slight effect might be explained simply by the intrinsic differences between the treatment and control groups. In any case, the findings of both methods are consistent in pointing out that the CDH programme did not create strong disincentives to work nor did it reduce the intensity of job search among the beneficiaries.

Therefore, once again, the estimated results imply that the programme achieves most of the expected objectives described as part of the theories of change (see page 270). This means that it is quite probable that the resources, activities, mechanisms and channels of change used are sufficient for the CDH to achieve high levels of efficiency in the medium and long term. However, it is also clear that there is still plenty of room for its implementation mechanisms to improve and achieve even better results, especially in terms of human capital investment and unemployment reduction.

By means of two different quantitative methods, it has been confirmed that people who opted for the CDH programme (on at least one occasion) have experienced improvements in terms of welfare, generation of human capital and housing conditions;

while they practically have not reduced their willingness to work and send their children to school at all. In short, these results suggest that the presence of conditionalities is *not* necessary for social assistance programmes to achieve most of their expected goals. At least in the Ecuadorian case, the recent implementation of a programme of unconditional cash transfers, as an alternative to the traditional CCT programme, has had very good results and has far exceeded expectations. The presence of conditions can only be justified by the estimated 3 percent reduction in the probability that the first-born children enrol and attend school when people choose the CDH programme.

In order to strengthen the findings of this research, the analysis is complemented in the next section by measuring the effects of the CDH on welfare level and the other outcome variables using an alternative quasi-experimental method that also addresses the possible endogeneity problems (i.e. instrumental variable technique).

Table 35: Diff-in-Diff Regression Estimates of the Effect of the CDH on the Welfare Index

(Standard Errors in Parentheses)

	OLS	OLS	OLS	OLS	OLS
Independent variable	(1)	(2)	(3)	(4)	(5)
Treatment Group Indicator	-0.0539*** (0.0207)	-0.3342*** (0.0199)	-0.3005*** (0.0178)	-0.3007*** (0.0178)	-0.1154*** (0.0178)
2014 Year Indicator	9.1360*** (0.0201)	8.9921*** (0.0193)	7.4700*** (0.0175)	7.4700*** (0.0175)	7.0388*** (0.0177)
CDH Effect (Treatment Group Indicator * 2014 Year Indicator)	1.5615*** (0.0291)	1.4774*** (0.0279)	1.2339*** (0.0251)	1.2338*** (0.0251)	1.2160*** (0.0249)
Household's Educational Attainment	---	2.2533*** (0.0064)	1.8386*** (0.0058)	1.8376*** (0.0058)	2.0480*** (0.0059)
Household's Floor Material	---	---	3.2147*** (0.0054)	3.2147*** (0.0054)	3.2337*** (0.0054)
Unemployment Indicator	---	---	---	0.3947*** (0.0721)	0.6319*** (0.0716)
Age	---	---	---	---	0.0843*** (0.0006)
Constant	14.3519*** (0.0489)	10.3913*** (0.0483)	0.5050*** (0.0465)	0.5086*** (0.0465)	-2.7192*** (0.0517)
Reported Province of Residence Dummies	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓
Number of Observations	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790
R-squared	0.3442	0.3958	0.5134	0.5185	0.5199

Note: Estimates are from linear regression models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)

Table 36: Household's Educational Attainment Diff-in-Diff Regressions (CDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
	Dependent Variable: Household's Average Educational Attainment							
Treatment Group Indicator	0.2209***	1.2472***	0.2212***	1.2476***	0.2209***	1.2472***	0.1207***	1.1283***
	(0.0043)	(0.0054)	(0.0043)	(0.0054)	(0.0043)	(0.0054)	(0.0044)	(0.0049)
2014 Year Indicator	0.0596***	1.0614***	-0.0343***	0.9661***	-0.0344***	0.9661***	0.1890***	1.2081***
	(0.0043)	(0.0046)	(0.0044)	(0.0042)	(0.0044)	(0.0042)	(0.0044)	(0.0054)
CDH Effect	0.0706***	1.0732***	0.0554***	1.0570***	0.0553***	1.0569***	0.0618***	1.0637***
(Treatment Group Indicator * 2014 Year Indicator)	(0.0062)	(0.0066)	(0.0062)	(0.0066)	(0.0062)	(0.0066)	(0.0062)	0.0066
Household's Floor Material	---	---	0.1978***	1.2187***	0.1976***	1.2185***	0.1837***	1.2017***
			(0.0013)	(0.0016)	(0.0013)	(0.0016)	(0.0013)	(0.0016)
Unemployment Indicator	---	---	---	---	0.6134***	1.8467***	0.4660***	1.5936***
					(0.0183)	(0.0338)	(0.0183)	(0.0293)
Age	---	---	---	---	---	---	-0.0463***	0.9546***
							(0.0001)	(0.0001)
Number of Observations	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 37: Household's Quality of Floor Material Diff-in-Diff Regressions (CDH)

(Standard Errors in Parentheses)

	Ordered Logistic (1)		Ordered Logistic (2)		Ordered Logistic (3)		Ordered Logistic (4)	
Independent variable	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio	Ordered Logit	Odds Ratio
Dependent Variable: Household's Quality of Floor Material								
Treatment Group Indicator	0.0242***	1.0245***	-0.0053	0.9946	-0.0054	0.9945	-0.0177***	0.9823***
	(0.0043)	(0.0044)	(0.0043)	(0.0043)	(0.0043)	(0.0043)	(0.0044)	(0.0043)
2014 Year Indicator	0.8073***	2.2418***	0.8030***	2.2323***	0.8030***	2.2323***	0.8321***	2.2983***
	(0.0044)	(0.0098)	(0.0044)	(0.0098)	(0.0044)	(0.0098)	(0.0044)	(0.0102)
CDH Effect	0.1823***	1.2000***	0.1748***	1.1911***	0.1748***	1.1910***	0.1761***	1.1926***
(Treatment Group Indicator * 2014 Year Indicator)	(0.0063)	(0.0075)	(0.0063)	(0.0075)	(0.0063)	(0.0075)	(0.0063)	(0.0075)
Household's Educational Attainment	---	---	0.2278***	1.2559***	0.2276***	1.2556***	0.2128***	1.2372***
			(0.0014)	(0.0018)	(0.0014)	(0.0018)	(0.0015)	(0.0018)
Unemployment Indicator	---	---	---	---	0.0871***	1.0910***	0.0705***	1.0731***
					(0.0185)	(0.0201)	(0.0185)	(0.0198)
Age	---	---	---	---	---	---	-0.0057***	0.9942***
							(0.0001)	(0.0001)
Number of Observations	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790	1,428,790
Reported Province of Residence Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓	✓	✓	✓

Note: Estimates are from Ordered Logistic models. All specifications include a constant. Odds ratio interpretation is included.
*** Significant at the 1 percent level (P value<0.01)

Table 38: Diff-in-Diff Regression Estimates of the Effects of the CDH on School Enrolment and School Attendance of the First-born Child

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)	Probit Regression (5)
Dependent Variable: First-born Child Enrolled in School Dummy					
Treatment Group Indicator	0.0922***	0.0560***	0.0561***	0.0561***	0.0264***
	(0.0065)	(0.0068)	(0.0068)	(0.0068)	(0.0068)
2014 Year Indicator	0.0538***	-0.1364***	-0.1522***	-0.1522***	-0.092***
	(0.0077)	(0.0081)	(0.0082)	(0.0082)	(0.0083)
CDH Effect (Treatment Group Indicator * 2014 Year Indicator)	-0.0584***	-0.0815***	-0.0839***	-0.0839***	-0.082***
	(0.0111)	(0.0116)	(0.0116)	(0.0116)	(0.0116)
Household's Educational Attainment	---	0.5741***	0.5696***	0.5696***	0.5446***
		(0.0037)	(0.0037)	(0.0037)	(0.0038)
Household's Floor Material	---	---	0.0330***	0.0330***	0.0293***
			(0.0023)	(0.0023)	(0.0023)
Unemployment Indicator	---	---	---	-0.0126	-0.0352
				(0.0397)	(0.0398)
Age	---	---	---	---	-0.016***
					(0.0003)
Constant	0.3832***	-0.7406***	-0.8392***	-0.8393***	-0.088***
	(0.0156)	(0.0178)	(0.0192)	(0.0192)	(0.0252)
Number of Observations	295,206	295,206	295,206	295,206	295,206
Dependent Variable: First-born Child Attending School Dummy					
Treatment Group Indicator	0.0901***	0.0551***	0.0553***	0.0553***	0.0256***
	(0.0065)	(0.0067)	(0.0067)	(0.0067)	(0.0068)
2014 Year Indicator	-0.0118	-0.2039***	-0.2212***	-0.2212***	-0.161***
	(0.0075)	(0.0080)	(0.0080)	(0.0080)	(0.0082)
CDH Effect (Treatment Group Indicator * 2014 Year Indicator)	-0.0574***	-0.0785***	-0.0810***	-0.0810***	-0.080***
	(0.0109)	(0.0113)	(0.0113)	(0.0113)	(0.0114)
Household's Educational Attainment	---	0.5498***	0.5449***	0.5449***	0.5202***
		(0.0036)	(0.0037)	(0.0037)	(0.0037)
Household's Floor Material	---	---	0.0361***	0.0361***	0.0324***
			(0.0023)	(0.0023)	(0.0023)
Unemployment Indicator	---	---	---	0.0003	-0.0224
				(0.0392)	(0.0393)
Age	---	---	---	---	-0.016***
					(0.0003)

Constant	0.3491*** (0.0154)	-0.7297*** (0.0175)	-0.8379*** (0.0188)	-0.8379*** (0.0188)	-0.087*** (0.0248)
Number of Observations	295,225	295,225	295,225	295,225	295,225
Province of Residence Dummies	✓	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)

Table 39: Unemployment Diff-in-Diff Regressions (CDH)

(Standard Errors in Parentheses)

Independent variable	Probit Regression (1)	Probit Regression (2)	Probit Regression (3)	Probit Regression (4)
Dependent Variable: Dummy for Being Unemployed				
Treatment Group Indicator	0.0358***	0.0250**	0.0254**	0.0065
	(0.0106)	(0.0106)	(0.0106)	(0.0107)
2014 Year Indicator	0.0169	0.0035	-0.0032	0.0635***
	(0.0105)	(0.0105)	(0.0107)	(0.0109)
CDH Effect (Treatment Group Indicator * 2014 Year Indicator)	0.0182	0.0155	0.0146	0.0177
	(0.0147)	(0.0148)	(0.0148)	(0.0149)
Household's Educational Attainment	---	0.1039***	0.1024***	0.0808***
		(0.0030)	(0.0030)	(0.0031)
Household's Floor Material	---	---	0.0143***	0.0113***
			(0.0033)	(0.0033)
Age	---	---	---	-0.0126***
				(0.0004)
Constant	-3.2189***	-3.4065***	-3.4534***	-3.0118***
	(0.0325)	(0.0332)	(0.0350)	(0.0377)
Number of Observations	1,428,790	1,428,790	1,428,790	1,428,790
Province of Residence Dummies	✓	✓	✓	✓
Marriage Status Dummies	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓

Note: Estimates are from probit models. All specifications include a constant.
*** Significant at the 1 percent level (P value<0.01)
** Significant at the 5 percent level (P value<0.05)

8.5 Instrumental Variable (IV) Results

While repeated pre- and post-treatment measures of household well-being can provide unbiased estimates under relatively weak assumptions, such data was difficult to obtain and it was only possible through the artificial construction of a panel type database (at the expense of fewer observations), and the re-calculation of the Welfare Index using the same non-linear principal component method in both periods. Consequently, an alternative approach to addressing self-selection bias is often required. This thesis develops for the first time a strategy of instrumental variables that can be used to estimate the effects of the CDH programme when only post-treatment well-being measures are available. Using only welfare data from the Registro Social 2014, I show that the instrumental variable strategy can provide estimates of model impacts that confirm the results of the difference-in-differences estimator provided that a valid instrument is used in the analysis.

As discussed in Chapter 3, instrumental variables (IV) estimators seek to overcome self-selection problems by identifying a source of enough variation in who decides to receive the accumulated cash transfers that is unrelated to the unobserved variables that influence the outcome measure. This requires an *instrument* that meets two specific identifying conditions. First, it should provide an adequately precise prediction of whether or not an individual has opted for the CDH programme. Second, the instrumental variable should be uncorrelated with the unobserved factors that influence the outcome variable, which in this case is the level of well-being.

Intuitively, it seems quite likely that *credit experience* in the past can provide an adequate instrument for the analysis. Fortunately, the 2008 Registro Social asked individuals if they had received private financial credit at any time until that date. Given that this occurred long before the implementation of the CDH treatment, it can be expected that this variable complies with the conditions to be a valid instrument. More specifically, those with previous experience in requesting and receiving credits from private banks will surely be more willing to leave the conditional transfer programme to request the transfers in an accumulated manner. Thus, becoming a good predictor of whether individuals opt

for the CDH programme. Moreover, the experience with credits in 2008 will hardly be correlated with factors that influence the level of well-being six years later.

There is one more thing worth noting about the instrumental variable strategy suggested in this thesis. The IV estimators can provide consistent estimates of CDH impacts, but these estimates may still be somewhat biased in finite samples. The magnitude of this bias depends on the size of the sample and, even more, on the amount of variation in CDH treatment status explained by credit experience. For example, Bound, Jaeger and Baker (1995) show that the bias can be quite considerable when the instrument is a weak predictor of treatment status. Therefore, in this case, the IV estimates of the impacts of the CDH are very sensitive to the choice of the instrument, and if this variable (i.e. credit experience) is poorly correlated with the treatment status, the IV estimates can be misleading. In view of this, I decided to complement the following analyses by performing statistical tests that verify the relevance and validity of the chosen instrument. The results of endogeneity- and weak instrument-tests are presented in the following section together with the IV estimates obtained from the model.

8.5.1 The IV Effects of the Crédito de Desarrollo Humano on the Welfare Index

As explained in the Measurement Models section in Chapter 7 (see page 284), I present here the results of a two-stage least squares (2SLS) procedure with a probit model in the first-stage regression using a credit experience dummy (i.e. access to credit before 2008) as the instrument.

$$CDH_i = \theta_1 W_i + \theta_2 CREDIT_i + \varepsilon_{xi}$$

Then, I estimate the main regression using the probit fitted values of the CDH indicator as an IV, rather than the usual linear projection.

$$Y_i = \beta_1 W_i + \beta_2 \widehat{CDH_i} + \varepsilon_{zi} \,,$$

$$\widehat{CDH_i} = \hat{\theta}_1 W_i + \hat{\theta}_2 CREDIT_i$$

This two-step procedure is first described by Wooldridge (2002), who claims that it produces a more accurate and unbiased estimate of the treatment effect (and one notably larger in magnitude).[90] Note that the first-stage regression could also be estimated by OLS. It would be a linear probability model and the coefficients could be theoretically interpreted as with other OLS models. However, since the dependent variable is binary, the OLS estimates will suffer from heteroscedasticity and would not constrain the predicted values to lie between 0 and 1. Therefore, it is better to use a probit model that explicitly models the probability and does not suffer from heteroscedasticity.

The regression results of four different specifications (which include different control variables) show that, once the endogeneity problems are addressed by using an IV strategy, the CDH programme has *positive* and statistically significant effects on well-being (see Table 40). In fact, the IV estimates of the CDH effect are quite large in terms of magnitude (specifically, between 22 and 15 index points). This is in line to the statement by Wooldridge (2002), who points out that the usual IV estimate is much larger in magnitude than the OLS estimate (but sometimes only marginally significant). However, the interpretation of the coefficients is somewhat different as will be seen later.

The effects of the instrumental variables on the probability of choosing the CDH are calculated in the first-stage of the model. Credit experience, educational attainment, floor material and unemployment have positive and significant effects according to all the specifications. The interpretation of the probit coefficients is not straightforward, but it is easy to note that the proposed credit experience indicator has the greatest effect on the probability of being affected by the CDH. Thus, it seems to be a valid instrument and its

[90] See Wooldridge (2002), pages 625-635.

coefficient indicates that an individual is more likely to opt for the CDH programme if he previously had access to some private credit. Moreover, in the second stage of the model, the estimated coefficients show that –apart from the positive effect of the CDH– educational attainment and floor material also positively affect the level of well-being. While, on the other hand, being unemployed has a negative effect, but statistically insignificant.

The magnitudes of the CDH coefficients are large because the estimated CDH variable ($\widehat{CDH_i}$) that is used in the second-stage of the model is no longer a dichotomous variable, but a continuous one. That is, the probit fitted values of the CDH indicator no longer take values of 0 or 1, but any value between 0 and 1. Since it is an estimation based on the indicator of credit experience (and other instruments), the values taken by the estimated CDH variable are equal to the predicted probability of an individual choosing the CDH programme. Therefore, to calculate the estimated effect of the CDH or the estimated Welfare Index of a given individual, the CDH coefficient must be multiplied by his predicted probability of requesting the programme (e.g. 15.07 x Probability of CDH). The greater the predicted probability of requesting the CDH, the greater the estimated effect of the CDH on the Welfare Index.

For example, in the first stage of the model, taking as a reference the last specification and holding the values of all the predictors –apart from credit experience– constant at zero, the predicted probability of choosing the CDH programme for individuals that had access to private credit in the past is given by

$$F(constant + \ Credit\ Experience\ coef.) = \ F(-0.9254 + 0.2799) = 0.2593$$

where F is the CDF of the standard normal. Now, in order to calculate the estimated effect of the CDH on well-being, this probability should be multiplied by the CDH coefficient estimated in the second stage of the model:

$$Effect\ of\ the\ CDH = \ 15.0717 * 0.2593 = 3.90\ index\ points$$

Thus, the IV effect of the CDH on welfare is quite consistent with that estimated by the diff-in-diff method. Note, however, that the estimated effect will depend on the values of all the predictors in the first stage of the model.

One thing that I might want to check after having fitted my model is whether or not the probit fitted CDH treatment is an endogenous variable. Therefore, I complement the analysis performing two different tests of endogeneity using the Durbin score and the Wu-Hausman statistics. The null hypothesis of these tests is that all explanatory variables are exogenous, in which case I could have used the regular OLS regression instead of the IV method. Both statistics have very small P-values (i.e. close to zero), which indicates that the null hypothesis that dependent variables are exogenous should be rejected. Consequently, the implementation of an IV strategy was necessary since it is confirmed that CDH treatment is an endogenous regressor due to self-selection of beneficiaries.

Finally, I would like to know if the chosen instrument (i.e. credit experience) is weak or not. This can be done by looking at the correlation between the instruments and the instrumented endogenous variable, and also by performing an F-test that indicates whether a group of variables are jointly significant. Basically, the F-test compares the estimated model with a model that contains no predictor variables (also known as an intercept-only model), and decides whether the added regressors improved the model. The F-statistic is the result of this test where the null hypothesis is that all of the regression coefficients are equal to zero. Therefore, the null hypothesis in this case is that the chosen instruments are weak (i.e. that the model has no predictive capability).

As can be seen in Panel B of Table 40, although the R-squared of the regressions is not very high (around 0.03), the estimated F-statistics are much larger than any of the critical values found in an F-table and conveniently calculated by the STATA statistical package. In addition, the P-value of the F-statistic is very close to zero. Thus, we can safely reject the null hypothesis that the instruments are weak (i.e. the fit of the intercept-only model and model are equal). The results of the test suggest that credit experience before 2008 is a valid instrument for CDH treatment.

Table 40: IV Regressions of the Effect of the CDH on the Welfare Index

(Standard Errors in Parentheses)

	(1)	(2)	(3)	(4)
Panel A: Two-Stage Least Squares				
CDH Indicator	22.1353***	18.9785***	15.0751***	15.0717***
	(0.5232)	(0.5003)	(0.4248)	(0.4247)
Household's Educational Attainment	---	1.9795***	1.5476***	1.5483***
		(0.0193)	(0.0158)	(0.0158)
Household's Floor Material	---	---	3.7150***	3.7151***
			(0.0119)	(0.0119)
Unemployment Indicator	---	---	---	-0.2146
				(0.1458)
Panel B: First Stage Probit for CDH Indicator				
Credit Experience	0.2958***	0.2835***	0.2799***	0.2799***
	(0.0054)	(0.0054)	(0.0054)	(0.0054)
Household's Educational Attainment	---	0.0703***	0.0662***	0.0661***
		(0.0012)	(0.0012)	(0.0013)
Household's Floor Material	---	---	0.0286***	0.0286***
			(0.0012)	(0.0012)
Unemployment Indicator	---	---	---	0.0481***
				(0.0169)
R-squared	0.0305	0.0346	0.0352	0.0353
F statistic (P value)	3050.54	2775.85	2717.42	2718.35
	(0.000)	(0.000)	(0.000)	(0.000)
Reported Province of Residence Dummies	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓
Number of Observations	716,497	716,497	716,497	716,497

Note: All specifications include a constant.

*** Significant at the 1 percent level (P value<0.01)

8.5.2 The IV Effects of the Crédito de Desarrollo Humano on Other Outcome Variables

In order to estimate the IV effects of the CDH programme on the other outcome variables considered in this thesis, I use different two-stage procedures depending on the type of dependent variable (i.e. binary discrete or ordinal discrete). The instrument used in all cases remains the indicator of credit experience. Therefore, the first stages of the estimations will be similar to what was done in the previous section for the Welfare Index (i.e. probit models), while the second stages present different regression models (i.e. probit or ordered logistic) given that the analysed outcome variables are not continuous (see Table 41).

In the case of the indicators of school enrolment and attendance (which are dichotomous variables) it is necessary to use probit models in the second stage regressions. The most popular IV estimate for probit models with an endogenous binary regressor (i.e. CDH indicator) is the bivariate probit (also known as biprobit). This procedure fits maximum-likelihood two-equation probit models – either a bivariate probit or a seemingly unrelated probit (two equations) (StataCorp, 2015). The biprobit model can be used as an instrumental variable approach when both the outcome variable and the endogenous regressor are binary. In this case, I use a 'seemingly unrelated bivariate probit regression' to estimate the effects of the CDH treatment on the probabilities of school enrolment and school attendance of the first-born. Specifically, what I do is to jointly estimate the probit models of two equations using the 'biprobit' command in Stata.

The IV estimates presented in columns (1) and (2) of Table 41 suggest that the effects of the CDH on the probabilities of school enrolment and attendance of the first-born are actually statistically *insignificant*. In other words, being a beneficiary of the CDH programme does not seem to affect the likelihood of enrolling and sending the first-born to school. Moreover, the regression results show that the employed instrument is valid. It can be seen in the first-stage of both biprobit models that the coefficient of the instrumental

variable (i.e. credit experience) is positive and statistically significant, indicating once more that it is a good predictor of CDH treatment. In addition, the second-stages of the biprobit procedures indicate that both the household's educational attainment and the income proxy (i.e. floor material) positively influence the outcome variables. These results are quite consistent with the estimates obtained by previous methods (i.e. diff-in-diff and natural experiment) in the sense that the CDH programme does not present a considerable effect on the human capital investment of the first-born children, especially if we compare it with the estimated effects of other explanatory variables, such as the level of education or income.

Each biprobit model is complemented with the results of two different endogeneity tests. In column (1), one can see that rho (ρ), which describes the correlation between the error terms, is insignificant (P-value > 0.05). In other words, $\epsilon 1$ and $\epsilon 2$ are independent, which suggests that in this case there is *no* evidence of endogeneity problems. Therefore, this joint probit model could be collapsed into two separate models: one for the probability of school enrolment and another one for the probability of receiving the CDH treatment. This means that the IV estimates of this model could be actually interpreted as of two regular independent probit models (Cameron and Trivedi, 2009). Besides, the absence of endogeneity in the first biprobit (column 1) is confirmed by a likelihood-ratio test that $\rho=0$, which compares the likelihood of the full bivariate model with the sum of the log likelihoods for the univariate probit models. Since the estimated Chi-Square statistic is not larger than the critical values (and its P-value is greater than 0.05), it is not possible to reject the null hypothesis that the error terms are uncorrelated or independent. Knapp and Seaks (1998) show that this test of whether $\rho=0$ can be used as a Hausman endogeneity test to confirm (or discard) the appropriateness of the biprobit IV method.

On the other hand, when the dependent variable is the indicator of school attendance in the second biprobit model (column 2), both the likelihood-ratio test and the statistical significance of rho (ρ) suggest the presence of endogeneity problems. More specifically, the error terms ($\epsilon 1$ and $\epsilon 2$) are not independent, which indicates that in this case CDH treatment should be effectively treated as endogenous. Therefore, these tests

confirm the pertinence of the biprobit model and the regression coefficients must be interpreted as of a joint bivariate model composed of two correlated probit models.

The same two-stage procedure is used in the case of the unemployment indicator as the outcome variable (column 3). The biprobit IV estimates show that apparently the impact of the CDH programme on the probability of being unemployed is negative and statistically significant. In other words, being a CDH beneficiary reduces somewhat the likelihood of unemployment. The interpretation of the coefficients of a bivariate probit model in terms of probabilities is a bit complicated. The specific effect of the CDH depends on the predicted probability of requesting the programme. However, by using the CDF of the standard normal and making some assumptions, it is possible to calculate the effect of the CDH manually and determine that the accumulated cash transfers in fact decrease the probability of being unemployed, but only in around 0.094 percentage points $[F(-3.0624) - F(-3.0624 - 0.7688)]$. This is, of course, assuming that the probabilities of requesting the programme could be only 1 or 0, and holding the values of all the other predictors in the second-stage probit model constant at zero. These results are once again consistent with those previously obtained by other methods. The natural experiment, diff-in-diff and IV estimates indicate all that the effect of the CDH on unemployment is too small to be taken into account or/and statistically insignificant.

In the first-stage of this model, the coefficients of the instrumental variable and the other regressors are positive and statistically significant, suggesting that they are valid predictors of the CDH indicator. The different endogeneity tests confirm the presence of an endogenous binary regressor due to self-selection. Thus, coefficients must be interpreted together as of a joint bivariate model. In the second-stage, the IV results also indicate that the household's educational attainment and floor material positively affect the probability of unemployment. Although the magnitudes of the effects are small, these estimates suggest that individuals with a higher level of income or educational attainment seem to be more unemployed. One possible explanation could be that only people with a certain level of well-being can afford to be unemployed for long periods of time. Most

individuals in a situation of extreme poverty would settle for almost any job after some time of being looking for an opportunity in the labour market.

Finally, I estimate the IV effects of the CDH on two of the main well-being components (i.e. household's educational attainment and floor material). Given the ordinal discrete nature of the outcome variables, in this case it is necessary to use ordered logistic models with an endogenous dichotomous variable in the second-stage regressions. More specifically, I use the probit fitted values of the CDH indicator as an IV, and then, the effects of the programme are estimated using the standard two-step procedure in ordered logistic models. The IV estimates in columns (4) and (5) confirm that the cumulative cash transfers considerably improve the probabilities of being in higher educational and floor material categories. The CDH coefficients are positive and statistically significant in both cases. However, it is important to keep in mind that they must be interpreted as ordered log-odds estimates and the CDH effect depends also on the estimated probability in the first-stage of the model, for example, the odds of being in a higher educational category increases in 0.18 times the predicted probability of requesting the CDH.

Overall, the IV results suggest that, once the possible endogeneity problems are solved by different two-step estimation procedures, there are significant positive effects of the CDH programme on the Welfare Index and its components. On the other hand, the IV effects on the probability of unemployment, school enrolment and school attendance are insignificant (either statistically or economically). Besides, most of the endogeneity tests support an endogeneity condition of the CDH indicator. Thus, failing to control for endogeneity would yield biased estimates on the effects of the programme so the introduction of quasi-experimental estimation methods in this thesis is largely justified.

Note that these results are for the most part very similar to those obtained using the difference-in-differences method. Moreover, the results of the three different methods used (i.e. natural experiment, diff-in-diff and IV) are consistent with the rationalist perspective on cash transfers. In general, all of them support the notion that people living

in poverty should be trusted in their ability to choose rationally what is most convenient for them and, therefore, social assistance should be in the form of unconditional cash transfers. On the other hand, the results are inconsistent with the widespread notion that cash transfer programmes need attached conditions in order to be effective (i.e. paternalistic approach) and, even more opposed, to the idea that poverty is caused by the very efforts to alleviate it (i.e. welfare dependency theory).

However, apart from the absence of conditions, there are other important elements that characterize the CDH programme and where their strengths may lie. Instead of forcing poor households to modify their behaviour, as a mechanism to overcome poverty, the *accumulated cash transfers* aim to generate productive processes that help them to link to the formal economy and develop their investment capacity. Besides, a true productive inclusion of those in poverty may only be achieved through confidence in their own capabilities. This element of *trust* is accompanied by different *complementary policies* (such as technical assistance and public education) that allow their rapid integration into the market economy and ensure their access to knowledge and technology. It could be the combination of these three elements (i.e. lump-sum cash transfers, trust and complementary practices) together with unconditionality that make the real difference. This possibility and the different conclusions that can be inferred from this research project will be discussed in depth in the last chapter of this thesis.

Table 41: IV Regressions of the Effect of the CDH on Different Outcomes

(Standard Errors in Parentheses)

	School Enrolment (1)	School Attendance (2)	Unemployment (3)	Educational Attainment (4)	Floor Material (5)
	Panel A: Second Stage IV Estimation				
	Probit Regression	Probit Regression	Probit Regression	Ordered Logistic	Ordered Logistic
CDH Indicator	0.1252	0.1380	-0.7688***	0.1877***	0.1037***
	(0.1209)	(0.1209)	(0.1236)	(0.0075)	(0.0071)
Household's Educational Attainment	0.4415***	0.4062***	0.1185***	---	0.2375***
	(0.0075)	(0.0083)	(0.0036)		(0.0021)
Household's Floor Material	0.0328***	0.0399***	0.0253***	0.2122***	---
	(0.0040)	(0.0040)	(0.0042)	(0.0019)	
Unemployment Indicator	-0.1165*	-0.0923	---	0.6414***	0.0879***
	(0.0628)	(0.0614)		(0.0253)	(0.0260)
	Panel B: First Stage Probit for CDH Indicator				
Credit Experience	0.2994***	0.2994***	0.2798***	0.2901***	0.2836***
	(0.0125)	(0.0125)	(0.0054)	(0.0054)	(0.0054)
Household's Educational Attainment	0.0682***	0.0682***	0.0661***	---	0.0701***
	(0.0044)	(0.0044)	(0.0012)		(0.0012)
Household's Floor Material	0.0229***	0.0229***	0.0286***	0.0377***	---
	(0.0033)	(0.0033)	(0.0012)	(0.0012)	
Unemployment Indicator	0.0263	0.0263	---	0.0788***	0.0494***
	(0.0537)	(0.0537)		(0.0169)	(0.0169)
Rho (ρ)	-0.0816	-0.1529**	0.5242***	---	---
Chi-Square Statistic/ (P value)	1.1997	4.1403	18.2933	---	---
	(0.2734)	(0.0419)	(0.000)		
Reported Province of Residence Dummies	✓	✓	✓	✓	✓
Reported Marriage Status Dummies	✓	✓	✓	✓	✓
Ethnicity Dummies	✓	✓	✓	✓	✓
Number of Observations	101,595	101,595	716,497	716,497	716,497

Note: All specifications include a constant.

*** Significant at the 1 percent level (P value<0.01)

** Significant at the 5 percent level (P value<0.05)

* Significant at the 10 percent level (P value<=0.1)

CHAPTER 9
Conclusions: Does Trust Make a Difference?

9.1 Introduction

After taking the reader on a long journey of discovery about cash transfer programmes and conditionality, this expedition draws to an end. The aim of this final chapter is to summarize the content, recapitulate the most important findings, examine the methodological, empirical and theoretical contributions, as well as draw conclusions of a more general nature that can be inferred from this thesis. A more detailed explanation of the impact evaluations conducted and the estimates obtained has been provided in each substantive chapter, and therefore, these will not be repeated here.

This chapter begins by comparatively analysing the effectiveness of the Bono de Desarrollo Humano (BDH) and the Crédito de Desarrollo Humano (CDH) programmes. The empirical evidence obtained is analysed together to determine which of them has achieved the best results in terms of welfare generation, human capital investment (i.e. investment in children's education) and labour supply. Moreover, the policy implications of these results are discussed, with special emphasis on the political and academic debates about the true efficacy of these welfare programmes and the issue of conditionality.

Specifically, practical importance of attaching conditions to cash transfers is examined in depth and some general conclusions are drawn.

This is followed by the analysis of whether it is effective to set aside conditionality and trust those in poverty. In fact, trust could be a key element for the design of new and improved forms of social assistance. Therefore, this discussion will be important in determining whether programmes such as the CDH –which introduce elements of trust together with complementary policies within social assistance practices– actually represent a step forward in the global challenge of designing and implementing more efficient mechanisms to fight poverty and make the poor self-sufficient.

The third part of the chapter discusses the methodological limitations of this research project. Like all quantitative impact evaluations, the most important constraint of this study is that it only quantifies observable changes in the outcome variables, but does not explain the treatment effects. In other words, although the estimates obtained indicate whether a policy is effective, they do not explain in detail the underlying reasons behind the relative success (or failure) of social assistance programmes. Nevertheless, as will be seen below, some significant conclusions can be made. The chapter concludes with recommendations for future research.

9.2 Effectiveness and Policy Implications

The data used in this thesis comes directly from two Registro Social databases (i.e. 2008 and 2014). The Registro Social is an information system for public policies that processes data collected every six years by the Ministerio Coordinador de Desarrollo Social (MCDS), mainly in the areas with the highest poverty rates within the country. It provides comprehensive information on individual- and household-level socio-economic characteristics, enrolment in social assistance programmes and labour supply. Undoubtedly, this information system has had very important developments in terms of technology, methodology and implementation capacity during the last decade. However,

the difficulties encountered in this research project to artificially build a panel-type database of individuals, and to construct the necessary variables for the analysis, highlight the importance of developing better information systems (i.e. unified, continuous and with higher levels of coverage) that allow a more effective targeting of social programs for poverty reduction, more accurate and frequent evaluations of their results and, consequently, substantial saving of resources for the government.

Different quasi-experimental methods (i.e. natural experiment, difference-in-differences and instrumental variable) were proposed to estimate the effects of the programmes on a number of outcomes related to well-being, human capital investment and labour supply. The use of these methods, together with specific evaluation designs for each programme, constitute the methodological contribution of this project to an evaluative literature where experimental methods have predominated. Through them, the impacts of the traditional BDH programme and the non-conditional CDH programme were estimated and discussed at length in Chapters 6 and 8 respectively. This body of empirical evidence responds to the first research question that guided this enquiry.[91]

Building on previous chapters, we can now compare the effectiveness of these two types of cash transfer programmes. By contrasting the estimated effects on different outcomes, it is possible to empirically contribute to the policy debate about the true efficiency of these welfare interventions as mechanisms to help overcome poverty. In addition, these findings have important implications on the long-standing debate about conditionality. More specifically, taking into account the estimates obtained, the role that conditions play for the success of social assistance programmes is also analysed below. This analysis constitutes one of the most important theoretical contributions of this thesis and, at the same time, responds to the second research question regarding the necessity/sufficiency of conditionality.

First, the empirical results consistently show that the BDH programme of conditional cash transfers (CCTs) has no effects (or even has small negative effects) on

[91] The research questions addressed in this research project are laid out on page 58.

the Welfare Index, the average educational attainment of the households and the housing conditions of the people living in poverty (i.e. quality of floor material as an income proxy). On the other hand, the different methods indicate that the CDH programme has considerable positive impacts on the overall welfare situation of the poor and on the two components of welfare considered in this thesis. Table 42 summarizes regression estimates on welfare-related outcomes.

It was established in Chapter 2 that there is some evidence in the existing evaluative literature suggesting that the BDH programme had significant positive outcomes –during the first half of the past decade– in terms of reducing monetary poverty (León et *al*., 2001) and improving food consumption (Ponce, 2008) (see page 65). However, it has also been pointed out that no studies have been conducted lately on the impact of the BDH programme on welfare, mainly due to the lack of experimental data and possibly also because its effectiveness has been taken for granted. The findings of this research project visibly indicate that at least in the current decade the traditional CCT programme in Ecuador has failed to deliver the promised results in terms of welfare, namely, it has not improved lately the quality of life of those in poverty. Instead, it seems that it has been the recently implemented programme of unconditional and cumulative cash transfers that has brought about several promising results in this respect, despite the short time that has passed since its implementation and the little (or non-existent) attention/support it has received from a considerable portion of tax-payers, politicians and multilateral development institutions.

Regarding the current policy debate on the effectiveness of the CDH, these results strongly support the arguments in favour of the implementation of this programme made by the Ecuadorian government through different institutional reports (IEPS, 2014; MIES, 2013b) – and reinforce the evidence presented by a few existing perception studies (Martínez & Mariño, 2013; Coba & Diaz, 2014) (see page 101). Although there were no significant impact evaluations on this programme, government officials and some academics have argued that the CDH builds capacities and provides opportunities to overcome poverty. Indeed, the findings of this thesis indicate that the CDH cash transfers

(along with their complementary policies) are significantly improving both the living standards of poor households and their average educational attainment. Moreover, as suggested by the government and most perception studies, beneficiaries seem to be effectively using the transfers (e.g. in productive investments), which would explain their improved levels of well-being.

On the other hand, those concerns about the operability and efficiency of the CDH programme raised by Maldonado and Moreno (2011) and Ponce (2013) –and supported by the evidence put forward by Bermeo (2013) and Castillo et *al*. (2017)– do not find sustenance in my results (see page 103). From a paternalistic perspective, they argue (for example) that transfers may not be used in productive activities and, therefore, they recommend implementing control mechanisms (i.e. conditions). These ideas presuppose that those in poverty are not capable enough to manage accumulated cash transfers or start their own businesses. However, in the light of the findings of this thesis, it seems quite unlikely that a significant part of the money is not being used productively (e.g. in consumption of temptation goods or debt payment). Thus, it does not seem necessary to implement any type of investment-related conditions. Similarly, other practical concerns found in the literature seem to be put to rest: transfers assignment through self-selection and without major requirements (apart from being poor) seems to be an appropriate targeting mechanism because the transfers are reaching the target population; the quality and frequency of the training workshops seem to be at least sufficient for the beneficiaries to start a business or improve their living conditions; and the absence of education-related conditionalities does not seem to be a major problem (at least) in terms of welfare [92].

From a broader perspective, my results have important implications on the social policy debate about conditionality discussed in Chapter 1 (see page 45). Contrary to the paternalistic arguments in favour of conditions in anti-poverty programmes made by Bastagli (2008) and Fiszbein et *al*. (2009) –and to the evidence put forward by Rawlings

[92] In fact, the CDH programme positively affects investment in human capital (i.e. household's average educational attainment), possibly because beneficiaries acknowledge the importance of education for success in family businesses.

and Rubio (2005), Danvers (2010) and Behrman et *al*. (2010)–, these findings indicate that forcing or manipulating individuals to change their behaviour is not beneficial to them. More specifically, conditionality as a policy mechanism has not been effective for promoting well-being in recent years in Ecuador. Not only have the conditions not provided significant additional effects, but they seem to have been detrimental to the beneficiaries. The entire positive 'income effect' of the cash transfers (i.e. the fact that incremental income allows itself poor households to improve their quality of life) seems to be offset by a negative effect of conditionality. Since only the conditional BDH programme did not have a positive well-being effect, the 'conditionality effect' of the cash transfers must be negative or non-existent.

These results support the rationalist arguments against conditionality made by Freeland (2007), Hanlon et *al*. (2010) and Myamba and Ulriksen (2016) – and add to evidence presented by De Carvalho (2008), Haushofer and Shapiro (2013), Blattman and Niehaus (2014). In accordance with their ideas about conditionality, the findings of this thesis reflect that people living in poverty know better than the state how best to use their scarce resources and, therefore, they should be trusted in their knowledge and ability to make decisions that promote their own well-being. In fact, conditions seem to be imposing costly distortions (in terms of well-being) on people's rational behaviour and unjustified restrictions on their freedoms.

Hence, the empirical evidence clearly suggests that, in the case of Ecuador, the presence of conditionalities in social assistance programmes is neither *necessary* nor *sufficient* to ensure that families overcome poverty in a sustainable manner reaching higher levels of well-being, increasing their human capital and improving their housing conditions. In other words, conditions seem to play only a secondary (or even harmful) role in relation to the promotion of well-being among the poor, compared to other constituent elements of antipoverty programmes (such as the amount of the transfers, the trust between the parties, and the complementary policies).

Table 42: Summary of Regression Estimates on Welfare-Related Outcomes (Full Specification Results)

Method	Outcome	Effect of the BDH	Effect of the CDH
Natural Experiment	Welfare Index	Negative	Positive
	Household's average educational attainment	Negative	Positive
	Quality of floor material	Negative	Positive
Difference-in-Differences	Welfare Index	Negative (small)	Positive
	Household's average educational attainment	Negative (very small)	Positive (very small)
	Quality of floor material	Negative (very small)	Positive
Instrumental Variable	Welfare Index	---	Positive
	Household's average educational attainment	---	Positive
	Quality of floor material	---	Positive

Second, regression estimates on children's human capital investment outcomes are quite consistent among the different evaluation methods. On the one hand, the estimated effects of the BDH programme are always neutral or insignificant. On the other hand, the effects of the CDH programme are neutral according to two of the three methods used. The only method that estimates some statistically significant effect is that of difference-in-differences, according to which the non-conditional programme reduces somewhat the probabilities of school enrolment and attendance of the first-born child. However, it is

important to note that the magnitudes of these negative effects are quite small (i.e. less than 3 percentage points). Broadly speaking, the results show that neither of the two cash transfer programmes has a significant impact (positive or negative) on the probabilities of school enrolment and school attendance of the first-born child. Table 43 summarizes regression estimates on human capital investment outcomes.

The estimated neutral effects of the BDH may be surprising in light of the evidence in the evaluative literature supporting that this programme helped to increase school enrolment (Schady & Araujo, 2006; Dobronsky & Rosero, 2007) (see page 66). However, the previous impact evaluations were carried out before the most significant changes in the programme with respect to the amount of the transfers, the eligibility conditions of beneficiaries and the verification process of conditionalities (as explained in Chapter 2). Besides, there is also evidence indicating that the BDH has no effects on school enrolment in the second poorest quintile (Oosterbeek et *al*., 2008) and that the impacts on other children-related outcomes, such as their attendance at health centres, have been limited (Paxson & Schady, 2007).

The findings of this thesis in terms of school enrolment/attendance are consistent with (and could even explain) the estimated absence of positive effects of the BDH on well-being. If the conditional programme is not even 'positively modifying' the behaviour of those in poverty in the first place, it would be more difficult to observe long-term improvements in their welfare situation. Nevertheless, it is important to emphasize once again that the estimated results are based on investment decisions in the human capital of the first-born, who are traditionally responsible for helping parents to work (see page 178). Therefore, it could be the case that some effects of the BDH on schooling fall on the other children in the household.

In relation to the effectiveness of the CDH, most of the findings indicate that unconditional cash transfers improve the welfare level of the poor without negatively affecting the level of schooling of the children. The estimated neutral effects of the programme on children's human capital investment are contrary to the arguments

formulated by some critics of the CDH initiative (see page 103). Specifically, it has been posed in the academic literature that the accumulated and unconditional cash transfers of the CDH may be encouraging children not to attend school or health centres, since there are no control and enforcement mechanisms to modify parent's behaviour. In addition, the new businesses undertaken by the parents need cheap labour that the children (especially first-born) could be providing (Maldonado & Moreno, 2011; Ponce, 2013).

Thus, the estimated results suggest that this practical concern about the CDH does not represent a serious problem. However, the somewhat different findings obtained using the difference-in-differences method show that the possibility of negative repercussions on children's schooling is not an unfounded concern that should be taken lightly either. According to these specific results, it is possible that the delivery of cash transfers without education-related conditionalities, together with the promotion and support of productive business activities among the poor, have a slight but negative effect on the schooling of the children. Consequently, it will be important to continue evaluating the impacts of the CDH programme on human capital investment decisions and child labour in the near future.

The results on school enrolment also have implications in the debate on conditionality. Contrary to the evidence put forward by Fiszbein et *al.* (2009) and Danvers (2010) –and adding to evidence presented by Ozler et *al*. (2010) and Arnold et *al.* (2011)– these findings indicate that conditionality alone doesn't always have additional effects on parental decisions regarding the schooling of their children. In this case, cash transfers attached to conditions have not even been sufficient to achieve its objective to "positively modify" the behaviour of the beneficiaries. Therefore, if we assume that influencing the decision-making of those in poverty is the right path, it would be important to accompany conditions with complementary policies, such as constant awareness campaigns or better control and verification mechanisms, which in Ecuador have been insufficient. Moreover, conditionality might be necessary in order to increase the probability of sending children to school (at least in the short-term), since the pure 'income effect' of unconditional cash transfers is not achieving it by itself.

The issue is that, although conditions may be necessary to compel the poor to send their children to school, this obligatory act does not guarantee improvements in well-being (apart from being ethically questionable) due to the opportunity costs and the shortage of public services. In other words, more children attending school does not necessarily mean that they are receiving a quality education that will make a difference in the future. Besides, it is necessary to take into account that increasing children's school enrolment is not the mechanism through which the new generation of social assistance programmes (like the CDH) seek to provide sustained escapes from poverty. This is undoubtedly a desirable long-term outcome, but one that is easier to achieve as a result of productive inclusion and higher well-being levels among those in poverty. It could be that what is perceived socially as 'better decisions' about schooling is a result and not a cause of well-being.

Table 43: Summary of Regression Estimates on Human Capital Investment Outcomes (Full Specification Results)

Method	Outcome	Effect of the BDH	Effect of the CDH
Natural Experiment	Probability of school enrolment	Neutral	Neutral
	Probability of school attendance	Neutral	Neutral
Difference-in-Differences	Probability of school enrolment	Neutral	Negative (small)
	Probability of school attendance	Neutral	Negative (small)
Instrumental Variable	Probability of school enrolment	---	Neutral
	Probability of school attendance	---	Neutral

Finally, the estimated results show that neither of the two cash transfer programmes considerably affects the probability of being unemployed. In other words, social benefits are not generating *noteworthy* differences in the labour supply of BDH and CDH beneficiaries with respect to poor individuals who do not receive any type of benefit. Although some statistically significant effects were estimated, their magnitudes are very small. The impacts of the BDH on unemployment are consistent among the different evaluation methods, while those of the CDH are somewhat heterogeneous. Overall, the results suggest that these welfare programmes do not create strong work disincentives among people living in poverty. If anything, they are slightly decreasing the probability of being unemployed. Table 44 summarizes regression estimates on labour supply.

It has been established that there is not much empirical evidence about the disincentive effects of welfare programmes on adult labour supply in developing countries (see page 57). The lack of sufficient evidence has led to a broad debate about labour market engagement of cash transfers beneficiary households. Proponents of the welfare trap perspective argue that beneficiaries develop a 'culture of dependency', since they get used to the state providing for them. As a result, there may be less job search intensity or less willingness to accept low-paying jobs among cash transfer recipients (Blank, 2003). Contrary to these well-known assertions of the welfare trap (also known as the unemployment trap) theory made by Gilder (1981), Danziger et *al*. (1981), and Guzi (2013) among others –and reinforcing the evidence against them put forward by Moffitt (1992) and Bourguignon et *al*. (2003)–, the findings of this thesis indicate that cash transfer programmes in Ecuador do not encourage inactivity or foster dependency on public assistance.

Moreover, the presence of work-related conditions has been widely suggested, specifically by supporters of the paternalistic approach, as a necessary element to ensure the effectiveness of social assistance programmes. This is why even traditional CCT programmes, which are almost always unconditional to work and time-unlimited, have also been accused of generating perverse disincentives to work (Guzi, 2013). However, the estimated results imply that conditions related to work are *not* always necessary. In

this case, no evidence was found that the traditional cash transfers of the BDH or the unconditional transfers of the CDH have a detrimental effect on labour force participation.

Table 44: Summary of Regression Estimates on Labour Supply (Full Specification Results)

Method	Outcome	Effect of the BDH	Effect of the CDH
Natural Experiment	Probability of unemployment	Negative (small)	Positive (very small)**
Difference-in-Differences	Probability of unemployment	Negative (very small)*	Neutral
Instrumental Variable	Probability of unemployment	---	Negative (very small)*

* The magnitude of the effect is less than 0.09 percentage points.
** The magnitude of the effect is less than 0.02 percentage points.

9.3 Conditionalities vs Trust: What do Those in Poverty Need?

The evidence presented in this thesis shows that the non-conditional CDH programme has considerably superior effects compared to the conditional BDH programme in terms of well-being and, thus, it represents a much more effective alternative to fight poverty in Ecuador. Contrary to the traditional paternalistic approach, according to which conditionality constitutes the key element for the success of any cash transfer programme, it has been demonstrated that behavioural conditions are not *always* necessary or sufficient to achieve the desired objectives. In other words, conditionality actually plays a secondary role compared to the function played by other constituent elements of the programmes. However, if conditions are not so important, what is the element that makes the difference?

That is, what is the possible mechanism through which the differences in the estimated effects of both programmes can be explained?

In order to answer these questions, it is necessary to begin by determining the main differences between the BDH and CDH programmes. There are at least three fundamental features that radically differentiate the CDH from traditional CCTs (such as the BDH): the amount and scheduling of the payments, the complementary services of technical assistance and training, and the implicit presence of trust between the parties. Out of these elements, what most attracts attention is undoubtedly the achievement of mutual trust between the state and the beneficiaries. Not only because it comes directly to replace the highly valued element of conditionality, but because –contrary to what one might suppose– trust is a very uncommon feature in most social assistance initiatives that have been recently implemented around the world, given that the implementation of different types of conditions has clearly prevailed. Therefore, although it is by no means the only possible scenario, it is quite probable that in this case the development of an element of trust (manifested through the cancellation of behavioural conditions) represents the main mechanism to explain the positive effects of the CDH programme.

The implications on conditionality of this thesis contrast with the great popularity of social assistance programmes linked to conditions, and the common presumption that they are "one of the most effective ways" to reduce poverty in less-developed countries (The Economist, 2005; World Bank, 2009). As established in the theoretical framework, there are indeed some theoretical arguments supporting conditionality and a good amount of empirical evidence that corroborates that CCTs have improved the quality of life of the poor (Fiszbein et *al.*, 2009; Danvers, 2010). However, it has also been established that the problem has long been to determine the specific contribution of conditionalities, which has led to a broad debate – what portion of the effect on well-being is due to the requirement on CCT programmes that parents send their children to school (and health centres), and what portion is due to the 'income effect'? (Freeland, 2007; Myamba & Ulriksen, 2016).

The findings of this research indicate that when cash transfers in Ecuador cause improvements, it is definitely *not* due to conditionality. More specifically, in this case, the presence of conditionalities does not positively contribute to promoting well-being (i.e. it is not sufficient). This means that, in general, conditions do not always guarantee that anti-poverty programmes are successful or effective. Conversely, conditioning transfers could even be counterproductive, as indicated by the rationalist perspective. Therefore, the continuous implementation of the conditional BDH programme for about 20 years –and the marked preference for it in terms of budget allocation, coverage and institutional support– is better explained by factors (i.e. theoretical arguments) not related to its actual effectiveness.

The political economy of funding the programme might be a good explanation in this case, given that international organizations –such as the World Bank or the IMF–often condition their financial support in developing countries to the exclusive implementation of CCT programmes, arguing that poor households maintain incorrect ideas about the profitability of investing in the human capital of their children (Freeland, 2007; Fiszbein et *al*., 2009). If the effectiveness of conditional or tied aid is highly questionable, the ethics of this practice of foreign intervention is even more doubtful. At least, it can be said that the World Bank is consistent in its institutional policies: they promote the implementation of conditions for the poor who receive the cash transfers, much as they impose conditions on the less-developed countries that receive their economic aid.

The influence of international organizations goes far beyond the choice of the type of programme. Most of the impact evaluations of CCTs conducted in the Latin American countries have been carried out by the same institution that finances these programmes, following always the same premises and normally using the same experimental method of evaluation (i.e. randomized control trials). This is precisely the case of Schady & Araujo (2006) and Oosterbeek et *al*. (2008) –two of the most well-known quantitative studies on the BDH programme in Ecuador– which were financed by the World Bank and, using the same experimental data, both supported the dominant policy paradigm.

If the evaluators are not precisely the most suitable, the appropriateness of the random evaluation method also raises some doubts. As reviewed in the methodological framework, randomized control trials (RCTs) have been subject to extensive criticism in recent years, due mainly to ethical and methodological considerations (Devereux et *al*., 2013) (see page 111). In this sense, the different quasi-experimental methods used in this thesis constitute in themselves a methodological contribution for the appropriate and principled evaluation of social assistance programmes. At least in the case of Ecuador, there is no other such exhaustive impact evaluation of the BDH programme, or that uses the same methodological approaches. While in the case of the CDH, it is –as far as I know– the first quantitative evaluation that employs such an extensive sample of individuals and that takes into account different outcome variables.

Another consideration of political economy that could partially explain the continued implementation of the conditional BDH programme –and the marked preference for it– is the common impression among Ecuadorian policymakers that taxpayers are more likely to support the delivery of transfers or subsidies to the poor if the aid is (at least theoretically) linked to some kind of compulsory effort on the part of the programme beneficiaries to eradicate poverty on their own (Adato & Hoddinott, 2007; Fiszbein et *al*., 2009). These conditions are thought to be more popular and to receive greater acceptance by the general public and the media when the required obligations have something to do with the children. Therefore, it is probable that the continuous implementation of the BDH programme in Ecuador, as well as the popularization of CCTs in Latin American countries, respond more to a political or electoral calculation than to the actual outcomes of these programmes in terms of poverty and inequality reduction.

Now, if the imposition of conditions that seek to modify the behaviour of those in poverty does not always guarantee the promotion of well-being, what is the alternative? Is it more effective to set aside conditionality and start trusting the poor? Note that the answers to these questions are part of the theoretical contribution of this thesis and respond to the third and last research question that guided this enquiry.

The findings show that the Ecuadorian anti-poverty programme that eschews conditionality and combines accumulated cash transfers with complementary public services, obtained much better results in terms of well-being promotion. The presence of conditions has not been necessary in this case, since the combination of different components (including an element of trust) has proven to play a much more important role. Therefore, in general, traditional conditions are not always indispensable for programmes to be successful or effective. In fact, it is possible to obtain better results by trusting people living in poverty.

It has been established that the CDH is part of a new generation of integrated social assistance programmes that have adopted an alternative approach to facilitate the sustained escape from poverty via productive inclusion, which is sought by linking different public sector policies and interventions (see page 15). The evidence suggests that the CDH programme has been successful in its aim to promote well-being among the poor through the combination of unconditional cash transfers and complementary policies. As mentioned at the beginning of this section, each of the programme's distinctive components may be responsible for a portion of the effects on the different outcomes. First, the delivery of accumulated (or lump-sum) cash transfers for an entire year allows beneficiaries to make productive investments –such as starting a business or buying a costly asset–, which in turn improve their income (approximated by household's quality of floor material) and living conditions. Thus, it is probable that access to lump-sum cash transfers is covering (at least partially) the great credit and financial needs of those in poverty, given that normally they do not meet the formal credit requirements demanded by private banks.

Moreover, the professional technical assistance –focused on the generation of knowledge and human capital– seems to be reducing the skills gap between those in poverty and the rest of the population, caused mainly by inequities in access to education. This would explain the estimated positive effect of the CDH not only on well-being, but also specifically on households' average educational attainment. However, the limited magnitude of the effect on education (and the null effect on unemployment) points to the

need to continue improving the training process and the technical support, so that the poor can develop competitive advantages that allow them to participate more actively in the labour market and be more successful in their ventures by investing more efficiently.

Finally, there is an implicit element of trust (reflected through the unconditional nature of the transfers) that might be responsible for most part of the effects. If conditionality has an effect, why not the absence of it? Much more when it is replaced by another element of a more positive nature. As stated in the theoretical framework, building a relationship based on trust is not easy, but it could be very beneficial (Bachmann & Zaheer, 2006). Trust is a complicated concept that depends on the actors (e.g. government and poor families), rules of the game (e.g. lump-sum cash transfers for productive investments), and context (e.g. Ecuadorian economic and political situation). Thus, the mere absence of rules (or conditions) does not necessarily imply the presence of trust.

However, it has been also established that when the decision to remove these rules is clearly voluntary and based on positive expectations, it is much easier to build trusting relationships between the programme implementers and beneficiaries. In this case, those in poverty may have noticed that there is actually trust in them by the government when they were offered an alternative social assistance programme that delivers cash transfers without demanding anything in return. In addition, the possibility of choosing between the traditional and the new programme without conditionalities is a clear invitation to modify the relationship between the parties and to generate mutual trust. Moreover, the necessary conditions for trust postulated by Bachmann and Zaheer (2006) (i.e. interdependence, vulnerability and risk) are met between the parties, and the decision to trust each other was taken voluntarily by all parties accepting the typical risks (i.e. opportunistic behaviour or non-compliance). Therefore, I strongly believe that a trusting relationship was built in the case of the CDH, which made people living in poverty more responsible and, subsequently, positively influenced well-being outcomes.

After all, the basis of social protection should be the transmission of a sense of partnership or inclusion (Freeland, 2007). Therefore, more attention should be given to

the element of trust in social assistance practices. Only in this way, future research will be able to determine the independent effects of trust and economic inclusion (instead of conditions) on poverty reduction, as well as the relative importance of building trusting relationships between programme implementers and beneficiaries, which are not achieved simply by eliminating traditional conditionalities of anti-poverty programmes.

Although greater emphasis has been placed on the potentially key role played by the trust element (and the absence of conditionalities), it is important to note that each of the constituent elements of the CDH programme (i.e. the amount/schedule of payments, the complementary services and the implicit presence of trust) represents in turn a different mechanism that could possibly explain the estimated effects on the well-being of people living in poverty. However, these mechanisms are not necessarily mutually exclusive and, therefore, each one could be responsible for a greater or lesser part of the effect. Therefore, the conclusion that positive and even better results could be obtained by leaving aside conditionalities and trusting the poor does not mean in any way that the mere presence of trust *guarantees* these better results, but that conditionality is not so fundamental as it has traditionally been considered. In other words, although there are some possible scenarios (or mechanisms) to explain the positive effects of the CDH programme in Ecuador, in none of them does conditionality play a key or important role.

9.4 Limitations of this Research

Like all statistical methods, the difference-in-differences and the instrumental variable approach used in this thesis have considerable limitations. Perhaps the most important is that although they measure the observed changes in the dependent variables, they cannot *explain* these outcomes. "The diff-in-diff methodology only quantifies observable changes over time; it does not put this 'treatment effect' into its full and proper context" (Devereux et *al*., 2013). Consequently, the findings of this thesis cannot determine with certainty, for example, the underlying reasons for the positive effect of the CDH on well-

being and whether the relative success of this programme is due *exclusively* to the absence of conditions.

As discussed previously, there are at least three CDH programme components that may have played a fundamental role for the achievement of the expected results: the lump-sum cash transfers, the complementary policies, and the trust between the parties. It is the combination of these constituent elements which seems to be helping people to escape poverty in a cost-effective and scalable way. Thus, it might not be just the absence of conditions that generates improved well-being results. It could be the impulse that those in poverty get by the accumulated cash transfers, or it could be the complementary practices that give them a real advantage, or it could simply be the introduction of an element of trust (which is not necessarily the same as the lack of rules) that signals to the poor the importance of making the right decisions for themselves, so they try harder to make sure that the transfers are used correctly.

These components are common in the new generation of social assistance programmes in developing countries. Unfortunately, the portion of the effects that corresponds to each one of them cannot be easily determined by quasi-experimental research designs. Therefore, the possibility arises of a new policy debate, opposite to that of conditionality, which I would call the 'trust debate' – what proportion of the increase in well-being is due to the lump-sum cash transfers, what proportion is due to the complementary services, and what proportion is due to the trusting relationship between parties? In order to answer these questions, it would first be necessary to take a closer look at the role played by trust in social affairs and study the circumstances under which it is possible and effective to build relationships based on trust.

Similarly, the results of the quantitative evaluation do not explain the *reasons* behind the null or negative effects of the conditionality on well-being in the case of the BDH programme. There is a possibility that the conditions attached to the transfers are not even modifying the behaviour of the poor. In fact, the results on school enrolment and attendance support this hypothesis. If this is the case, the control mechanisms for

compliance with conditionalities would not be working properly and it would be necessary to monitor whether the sanctions are actually being applied. However, rather than having a null effect, the conditionalities seem to be offsetting the positive 'income effect' of the BDH cash transfers, in which case the explanation could be completely different. A negative impact of conditionality on well-being could be explained by the financial and opportunity costs of compliance with conditions faced by those in poverty and/or by the scarce provision of public services. For example, due to the limited provision of public services, conditionalities could have an adverse effect by compelling beneficiaries to make decisions that go against their best interests, such as attending crowded health centres or spending considerable amounts of money on transportation.

Another important limitation of this thesis is related to the natural experiment that is proposed to estimate the effects of the BDH programme. The fundamental problem is that, in this particular case, the selection of beneficiaries may not be as random as it seems. As mentioned in Chapter 3, the key identification assumption (and the starting point) for the evaluation of the BDH is that the assignment to the treatment and control groups is "as if" random. Although some kind of empirical verification of the "as if" randomness assumption was carried out (comparing the treatment group with the control group in terms of its observable characteristics before the intervention), this test does not allow ruling out the existence of unobservable differences between the groups. In other words, it is still possible that, instead of relying on a randomization process, the allocation of transfers is actually the product of the interaction of actors in the social and political spheres. In fact, Chapter 2 identified some geographic, political and socioeconomic factors that possibly affect the programme's targeting process, thus generating considerable potential selection biases (see page 75).

Far from being something of little importance, this latent concern about the selection issue certainly casts doubt on the validity of the natural experimental proposed for the BDH and, therefore, that of its estimates. It could be argued that in a non-controlled experiment (such as this) it is almost never known with certainty that the mechanism that assigns treatments to individuals is purely random (Sekhon and Titiunik, 2012).

Additionally, in order to corroborate and strengthen in some way the results obtained, different econometric techniques were used as part of the natural experiment (such as the difference-in-differences and the IV). Unfortunately, adding controls, using instrumental variables or controlling for baseline values is not enough to completely solve the potentially significant self-selection and/or biased selection problems. Therefore, it is not possible to effectively solve this problem as part of this thesis and more research (perhaps of the qualitative type) is necessary on beneficiary self-selection and biased selection. However, there are certainly some solid reasons to consider that the estimated results *do* reflect the real effects of the BDH and CDH programmes, the main one being that the estimates are robust to the different specifications and techniques; specially the IV, which proposes an alternative and effective way of dealing with the endogeneity issues.

Although this research project is based on the Ecuadorian case and it cannot be guaranteed that the results will be repeated under different circumstances, the implications of the findings are largely generalizable. By saying that conditions are not necessary or sufficient, it does not mean that they are never effective, but that conditionality is not the only alternative to promote well-being and probably it is not the best either. The CDH programme represents, above all, a sign that better results can be obtained by trusting the poor and seeking their productive inclusion. Its positive outcomes show us that there are alternative ways to help those in poverty without imposing ethically-questionable rules that violate their fundamental rights. Undoubtedly, this initiative represents a step forward in the global challenge of designing more efficient mechanisms to overcome poverty and make people self-sufficient. Therefore, it will be important to continue evaluating, perfecting and replicating this type of social assistance policies in other developing countries, always taking into account that the design of programmes must respond to the nature and extent of poverty in each place.

9.5 Recommendations for Future Research

As part of a future research project, it will be important to determine exactly how BDH and CDH beneficiaries are using the monetary transfers; how does a trust-based relationship influence their behaviour; and to what extent the training workshops, professional assistance and technical support offered by CDH programme administrators are helping beneficiaries to succeed in their ventures.

These issues are important because in order to *explain* the positive impacts of the CDH programme and the absence of effects of the BDH programme, I have to know first if poor households are actually using the money "correctly" (for example, in their productive investments) or if they are spending it on day-to-day consumption (e.g. food, debts, temptation goods, etc.). There is the possibility that families are wasting a considerable part of the money on temptation goods, and therefore, the effects of the programmes on their well-being could be underestimated. Moreover, it would be interesting to have a better understanding of the beneficiaries' perception about both cash transfer programmes and the additional services offered by CDH programme administrators, since this is one of the most innovative features of this programme.

More specifically, there are a series of questions that arise from this analysis and that the quantitative evaluation methods used in this thesis cannot answer due to their natural limitations. Therefore, it will be important to complement this research in the near future with other type of evaluation methods that seek to clarify these topics. The most important questions worth considering for future research are the following:

- Are poor families actually using most part of the cash transfers for investments in productive activities or are they using them to buy consumption goods or to pay debts?
- How do they perceive the action of eliminating conditionalities once they enrol in the CDH programme? Do they see it as an act of trust from the government?

- Are beneficiaries attending the offered workshops regularly? Do they think that the complementary benefits of the CDH are necessary?
- Are the instructors sufficiently trained to deliver basic courses in finance and entrepreneurship? Do transfer beneficiaries feel that it is useful to ask for professional assistance?

Note that in order to answer these questions, it will be necessary to use *qualitative* methods of evaluation. In fact, interviewing some beneficiary families and programme managers could help to better explain the quantitative results of the study and to better answer the key questions of this research. Unfortunately, this could not be done as part of this thesis for time reasons, but I believe that this could be a good way to continue my research in the near future and that it is important to suggest these topics to other researchers interested in social assistance practices.

Qualitative research about the BDH and CDH programmes will be important because it will attempt to provide an accurate description of how people experience cash transfers. It will provide information about the "human side" of these social assistance programmes represented by people's beliefs, opinions, emotions and behaviours. Moreover, it will be helpful identifying intangible factors, such as social norms, gender roles, ethnicity, and culture, which might play an important role in determining the way people spend the money from the transfers and the way in which trust influences their well-being. Consequently, qualitative and quantitative research could be used together to help interpret and better understand the complex reality of social assistance in developing countries and the implications of the obtained quantitative results.

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Appendix A: Public Spending on Social Assistance Programmes in Ecuador

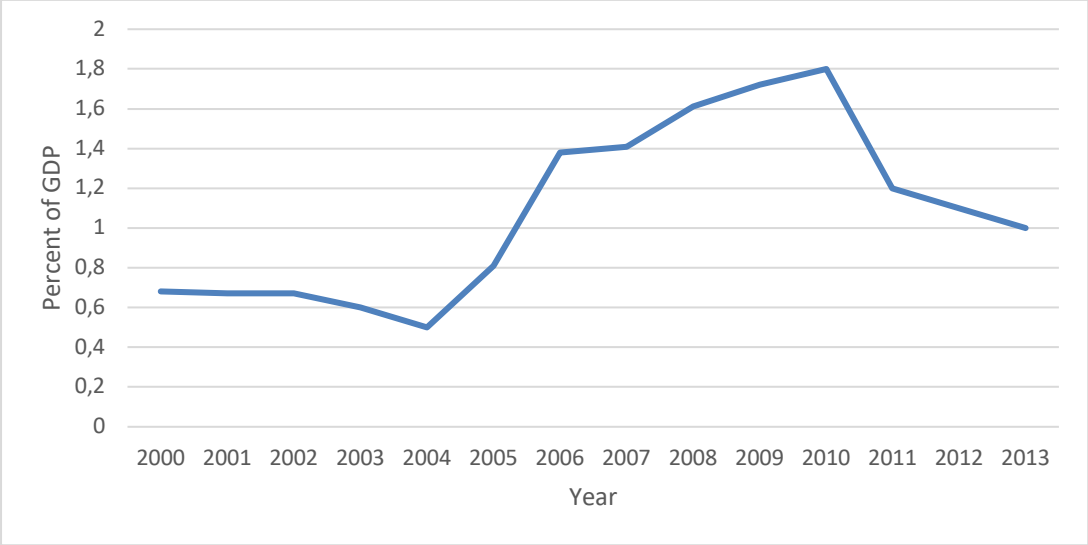
According to the latest available data from the Atlas of Social Protection - Indicators of Resilience and Equity (ASPIRE) database, the public spending on social assistance programmes in Ecuador (as percent of GDP) has recently experienced a reversal showing a considerable decrease since 2011 (see Figure A1), which surprisingly (or not) is contrary to the trend observed over the last years in most of the other countries in Latin America.

The ASPIRE database is the World Bank's compilation of Social Protection and Labour (SPL) indicators gathered from officially-recognized international household surveys in order to analyse the distributional and poverty impact of Social Protection and Labour programmes. ASPIRE includes a set of indicators based on nationally representative household surveys and administrative data. Programme-level administrative data on spending and number of beneficiaries in ASPIRE include both primary and secondary sources: official government reports; data provided directly by government officials; published World Bank country reports; and other international databases (World Bank, 2015b). The ASPIRE indicators based on household surveys are available for 112 countries and their sources include: household income expenditure surveys, Welfare Monitoring Surveys, Living Standard Measurement Surveys, and Surveys on Income and Living Conditions.

Social assistance (SA)/Social safety nets (SSN) expenditure imply the total cost of the programmes, including spending on benefits as well as administrative costs. The ASPIRE indicator captures both the capital and recurrent programmes' budgets and it is based on the administrative records of the programmes. The data on programme-level expenditure is available for 124 countries and it is presented as a percentage of GDP of the respective year. Expenditure indicators are usefully aggregated by type of programme categories (CCTs, UCTs, social pensions, in-kind transfers, school feeding, public works,

fee waivers, and other social assistance) for all social assistance programmes available in each country.

Figure A1: Public Spending Over Time on Social Assistance Programmes in Ecuador, as a Percent of the GDP



Source: Data from The Atlas of Social Protection - Indicators of Resilience and Equity ASPIRE database (Last Updated: 02/01/2018)

Looking at this data, it is apparent that between 2005 and 2010, Ecuador incurred an (average) expenditure on SA programmes of around 1.45 percent of GDP, which at that time placed this developing Latin American country above the average expenditure among the countries in its region (i.e. 1.3 percent of regional GDP). Moreover, as shown in Figure A1, the evolution of social assistance spending in Ecuador presented a fairly stable upward trend until 2010, so that its growth rate was one of the highest in South America, being surpassed only by the unusual rate shown by Colombia.

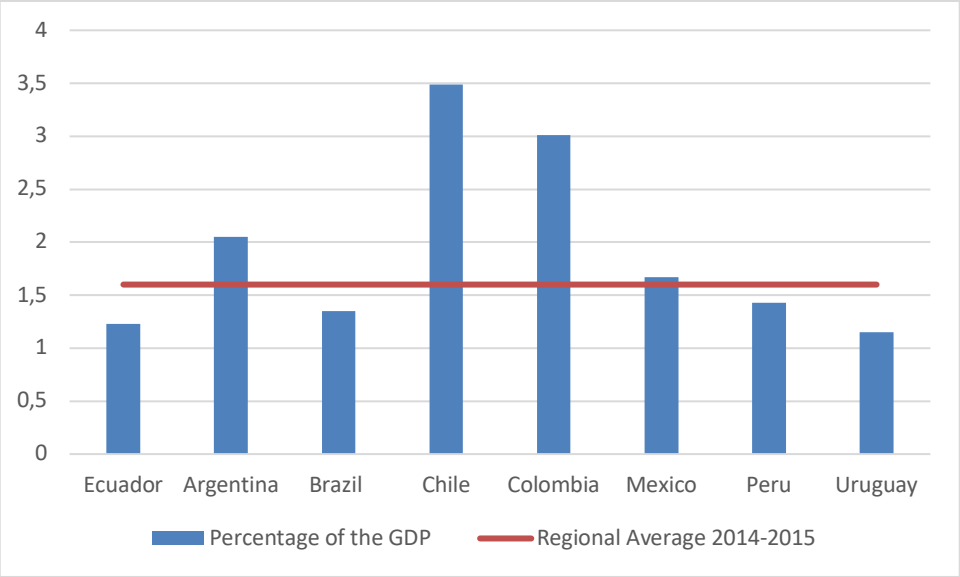
In fact, Ecuador's total social public expenditure maintained a growing trend in the first decade of the 21st century, reaching about 9.6 percent of the country's GDP in 2010.

Amongst this public spending, one of the components that grew the most was the spending on SA programmes, which include CCTs (i.e. BDH), UCTs (i.e. CDH, welfare pensions, and transfers for people with disabilities), and direct in-kind subsidies (mainly, school feeding programmes). At that time, Ecuadorian social assistance expenditure experienced a substantial increase, going from 0.6 percent of GDP in 2000 to 1.8 percent in 2010. Thus, at the end of the decade, the expenditure on social safety nets was around 60 percent higher than the average spending in Latin America, placing Ecuador in first place among the region.

However, as of 2011, an important decline in public spending on SA programmes has been experienced that currently places the country below the regional average for the period 2014-2015 (see Figure A2). More specifically, the ASPIRE data shows that social assistance spending in Ecuador was of 1.23 percent of GDP in 2015, while the average expenditure in Latin America was of about 1.6 percent. Therefore, it can be noted that recent spending trends in this type of programmes were contrary between Ecuador and the rest of the region or, put it another way, the visible reduction in SA spending that occurred in the last few years in Ecuador was not widespread in Latin America, but occurred specifically in this country, either for budgetary or strategic reasons.

In this regard, it should be noted that the recent trend of public spending on social assistance programmes (and in general in social protection) in the countries of Latin America has been ascending –or at least stable– in most cases, with Ecuador being the only country in the region that shows a persistent downward trend in social safety nets expenditure over the last few years. This reduction in public spending had, of course, its most significant repercussions on the Bono de Desarrollo Humano (BDH), since it is the main social assistance programme in the country (at least in terms of budget and number of beneficiaries).

Figure A2: Public Spending on Social Assistance Programmes in Some Selected Countries of Latin America (year 2015)



Source: Data from The Atlas of Social Protection - Indicators of Resilience and Equity ASPIRE database (Last Updated: 02/01/2018)

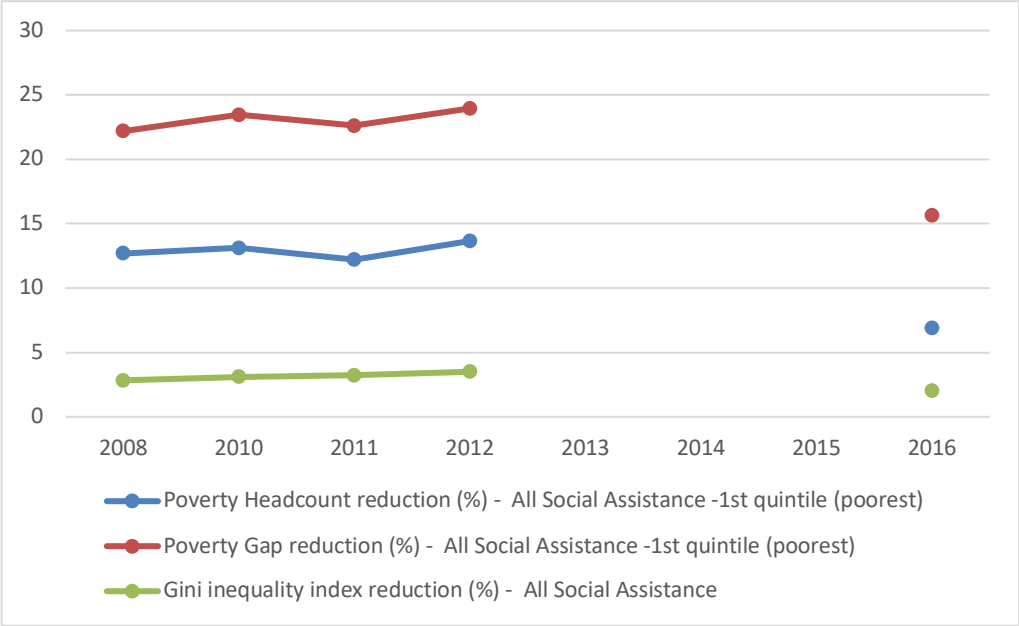
Given that the amount of BDH cash transfers actually increased in 2013 from 35 to 50 US dollars (see page 79), it was the level of coverage of the programme that was used as the adjustment mechanism, and therefore, the dimension most affected by the budgetary reduction. Recall that the number of beneficiaries of the BDH programme increased steadily since its implementation until 2011, when the level of coverage of the BDH began to experience a significant decrease precisely along with the reductions in public spending on this type of programmes. Specifically, the number of households receiving BDH cash transfers went from 1.21 million in 2011 to 1.05 million in 2013. Apparently, the level of coverage of the main social assistance programme declined progressively as of 2011 due to the constant reductions in the budget allocated to social assistance (see Figure 2).

Nevertheless, contrary to what might be expected, the considerable reduction in social assistance spending in Ecuador –and jointly in the coverage level of its main programmes–, which began in 2010 and has been maintained up to the present, did not lead to a significant reduction in the efficiency level of SA programmes. In fact, the effectiveness of these programmes in Ecuador – measured as their impact on the percentage reduction of poverty and inequality– remained practically unaffected until very recently (namely 2016), and it could even be said that the performance of social assistance slightly improved during the first years after the downward trend in public spending on SA programmes (see Figure A3).[93]

The ASPIRE data described in Figure A3 show that Ecuador's social assistance spending has maintained a good and almost constant performance in terms of poverty and inequality reduction from 2008 until at least the end of 2012, which is the last year with comparable information over time. During these five years, the reduction of the poverty headcount due to social assistance programmes in the first income quintile (poorest) was always between 12.6 and 13.6 percent. In addition, the reduction of the poverty gap due to SA throughout this period was between 22.1 and 23.9 percent. Finally, the decrease in the Gini inequality index was the most constant of all, maintaining an annual reduction caused by SA programmes of between 2.8 and 3.4 percent.

[93] In 2016, there was an important economic contraction in Ecuador -produced by exogenous factors and natural disasters- that caused serious social problems and, evidently, affected the percentage reduction of poverty and inequality (not only that caused by social assistance programs but in general). Thus, the efficiency in absolute terms of social assistance and social protection programs (percentage contribution to poverty reduction) was affected as well. However, its importance in relative terms (that is, with respect to other antipoverty policies) remained unchanged.

Figure A3: Poverty Headcount, Poverty Gap and Gini Inequality Index Reductions Over Time due to Social Assistance Programmes in Ecuador (latest years available), as Percentage of Pre-transfer Poverty and Inequality. [94]



Source: Data from The Atlas of Social Protection - Indicators of Resilience and Equity ASPIRE database (Last Updated: 02/01/2018)

In order to better understand the outstanding performance that the social assistance programmes implemented in Ecuador apparently had until 2012, it is useful to compare these figures with those of other countries in the region with similar characteristics and in the same period of time. According to the latest comparative data available for a group of eight countries chosen for this analysis (most corresponding to 2012), Ecuador's SA programmes achieved contributions to poverty reduction well above the group averages. Specifically, their estimated impacts were 13.6 percent on poverty headcount reduction,

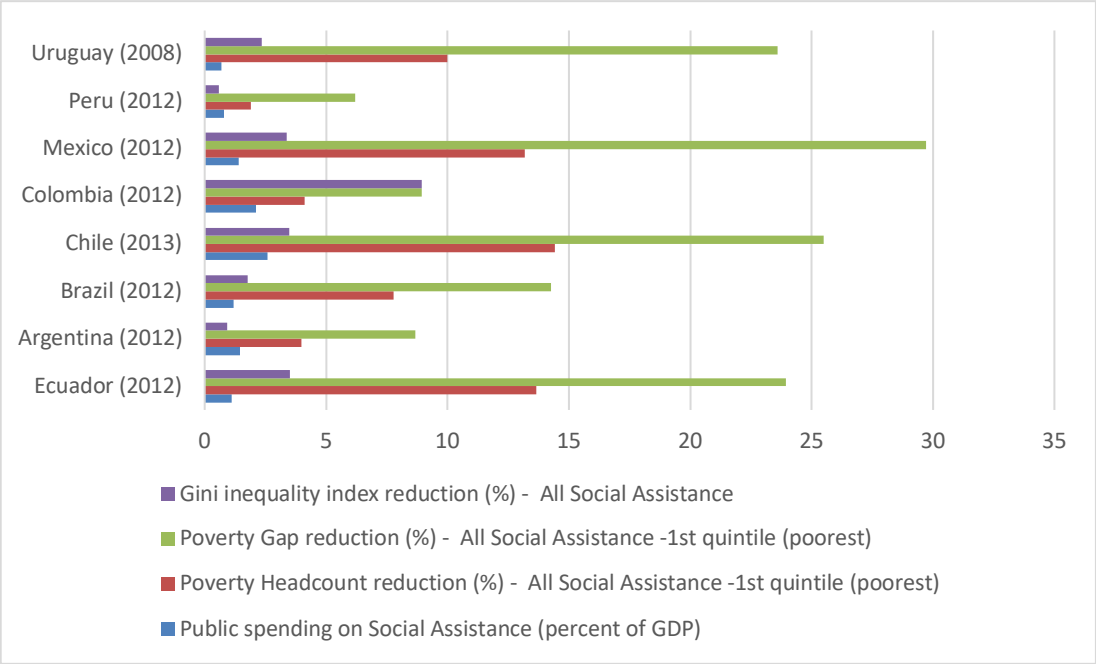
[94] The three measures are percentage reductions calculated in relation to the counterfactuals values, which are the poverty and inequality figures in the absence of social assistance programs.

23.9 percent on the reduction in the poverty gap and 3.4 percent on the reduction of inequality.

These results placed Ecuador (together with Mexico, Chile and Uruguay) among the countries with the most efficient social assistance programmes, at least in this group of countries for which comparable information is available (see Figure A4). In fact, it is interesting to note that what Ecuador achieved in 2012 in terms of poverty reduction is very similar to what was achieved by Chile –one of the most developed countries in the region–, but only using half of the Chilean public spending on SA, since in Ecuador it did not exceed 1.3 percent of the GDP.

Although the latest data available for Ecuador suggest a drastic reduction in the efficiency of social assistance programmes in 2016, these figures are not comparable with those of the previous years, since after 2015 the country went through a severe economic and social crisis, whose effects are still present. In fact, in the last few years, an unexpected process (for some people) of deceleration of the economic growth began –which later led to a prolonged economic recession– mainly caused by the international fall in oil prices, the appreciation of the US dollar (which is the currency used in Ecuador since 2000), the lack of a national monetary policy, and the different natural disasters that seriously affected the population (especially those in poverty) and the national economic performance (Pinza & Martín-Carrillo, 2016). Undoubtedly, all these external factors negatively affected the complicated process of poverty and inequality reduction that until then had been improving, as well as they limited the capacity of social assistance programmes accelerate this process (Ospina, 2015).

Figure A4: Impact of Social Assistance Programmes on the Percentage Reduction of Poverty Headcount, Poverty Gap and Gini Inequality Index (Latest Available Comparative Data).



Source: Data from The Atlas of Social Protection - Indicators of Resilience and Equity ASPIRE database (Last Updated: 02/01/2018)

The fact that the level of efficiency of social assistance programmes was not affected at least 3 years after the considerable reductions in public spending and in the level of coverage of the BDH programme, makes one question about the absolute and relative importance that CCTs have in the fight against poverty and inequality in Ecuador. In other words, given that social assistance in general –and BDH transfers in particular– apparently have been having a significant impact on different income poverty measures, the logic indicates that we should observe a significant reduction in their efficiency levels shortly after the number of beneficiaries of the programme was suddenly reduced by around 650,000 people (160,000 households). Since this does not happen, serious doubts

arise about the true effectiveness of CCT programmes (at least in Ecuador) and about the way in which this effectiveness is commonly measured.

There are some different alternatives to explain that the measured levels of efficiency of social assistance programmes have remained almost constant, despite the reduction in spending and coverage of the BDH. One possibility is that, in spite of reaching a smaller number of people, the targeting and selection mechanisms of the beneficiary households would have been improved in some way. However, this would mean that the filtering and under-coverage problems, which characterize this type of programmes, have been at least partially resolved in the last few years. This seems somewhat difficult to imagine when, according to different studies (e.g. Gutiérrez et *al*., 2013), these issues were notoriously present at least until 2011. Actually, the need to develop more sophisticated strategies so that CCTs reach the right people is considered a challenge that Ecuador still shares with other countries in the region (Ribe et *al*., 2010).

A second, much simpler, possible explanation could be that basically not enough time has passed -between the reduction in BDH spending and coverage levels and the latest available comparative data about SA efficiency- to be able to observe the real effect that these policy measures have had on the percentage reduction of poverty and inequality. However, we must bear in mind that, in this case, income poverty measures are used to determine the levels of efficiency; and also, that although CCT programmes are conceived as programmes that contribute to the alleviation of poverty in the long term, in the short and medium term they are important mechanisms to cope with price volatility by increasing the purchasing power of the families (Araya & Lizano, 2000).

Therefore, the effects of monetary transfers on the level of income (and consequently on income poverty) should have been present both in the short and long term, considering also that the participation of transfers in the monthly income of poor households in real terms is close to 23 percent (Gutierrez et *al*., 2013). In other words, such a significant reduction in the number of beneficiaries (around 160,000 households) of a transfer that provides families with an amount equivalent to 15 percent of the

minimum living wage in Ecuador (375 US dollars per month in 2017) should have had an almost immediate impact on any measure of poverty by income, especially in the first quintile of poverty. This is, of course, if other factors remain constant, including changes or implementations of other social assistance programmes at the same time, which evidently did not happen and this fact leads to a third and final possible explanation.

It is quite probable that the reduction of public spending has been accompanied by the inclusion of other social assistance programmes and policies, which in some way diminished the effect on income poverty. In fact, it is at the end of 2008 that the Crédito de Desarrollo Humano (CDH) programme was officially implemented in Ecuador with features that are very different from those of traditional cash transfer programmes. Besides, the number of beneficiaries of this programme increased precisely during the years in which the BDH coverage level decreases. According to the data published by the IEPS (2014), from 2008 to 2012, this programme delivered 1,025,348 credits throughout the Ecuadorian territory, which in monetary terms represented a total amount of 579,367,141 US dollars.

Therefore, this last explanation implies that the important role played by the BDH conditional cash transfer programme in Ecuador, at least in terms of *monetary* poverty reduction, was covered and perhaps surpassed by other social assistance practices that emerged precisely during the first years of reduction of public spending on SA. In other words, this would mean that, although the popular CCT programmes have proven to be effective in reducing certain poverty measures at least in the short term, they are not irreplaceable. Much less that their efficiency levels cannot be overcome by other types of programmes that do not currently have the same acceptance in governmental and international organization circles.

In the Ecuadorian case, the emergence of innovative policies and programmes that address other needs of poor families (for example, the need for productive credit, labour activation, technical assistance) as well as the immediate need for income, seems to show us a path different from that commonly used in developing countries (specially in Latin

America) to combat poverty in its different dimensions. The CDH is perhaps in this sense the most relevant of the recently implemented programmes, given that it presents a ground-breaking structure, which is very different from the one that has dominated for decades social assistance provision in the region.

Appendix B: Main Impact Evaluations of the Bono de Desarrollo Humano

Impact Evaluations						
Author(s)	Year of Application	Evaluation	Effect	Type of Effect	Level	Method
León et *al.*	2001	Impact of the Bono Solidario	Effect on consumption poverty reduction	Positive	Low	Non-experimental
Shady & Araujo	2006	Impact of the BDH	Effect on school enrolment	Positive	High	Experimental
Shady & Araujo	2006	Impact of the BDH	Effect on child employment	Negative	High	Experimental
León & Younger	2007	Impact of the BDH	Effect on child malnutrition	Negative	Low	Non-experimental
Martinez & Rosero	2007	Impact of the BDH	Effect on reduction of child labour	Negative	High	Non-experimental
Martinez & Rosero	2007	Impact of the BDH	Effect on schooling	Positive	High	Non-experimental
Paxon & Shady	2007	Impact of the BDH	Effect on children's health	Positive	Low	Experimental
Paxon & Shady	2007	Impact of the BDH	Effect on children's cognitive development	Positive	Medium	Experimental
Paxon & Shady	2007	Impact of the BDH	Effect on children's frequency of attendance at health centres	No effect	---	Experimental
Dobronsky & Rosero	2007	Impact of the BDH	Effect on school enrolment	Positive	Medium	Experimental
Ponce	2008	Impact of the BDH	Effect on household's food consumption	Positive	Medium	Non-experimental
Ponce & Bedi	2008	Impact of the BDH	Effect on academic performance	No effect	---	Regression discontinuity
Ponce & Bedi	2008	Impact of the BDH	Effect on school enrolment	Positive	Medium	Regression discontinuity
Oosterbeek et *al.*	2008	Impact of the BDH	Effect on school enrolment	Positive (first poorest quintile)	High	Regression discontinuity (experimental data)
Oosterbeek et *al.*	2008	Impact of the BDH	Effect on school enrolment	No effect	---	Regression discontinuity (experimental data)

Appendix C: Summary of Individual- and Household-Level Variables Included in the Constructed Registro Social Panel Dataset

Category	Variable of interest	Name of Variable in Database	Survey Question in 2014
Human Capital	Literacy	leeescribe	Section 5, Question 36: *Can you read and write?* (yes or no)
	School Enrolment	matricula	Section 5, Question 37: *Are you enrolled in an institution of regular education?* (yes or no)
		razonnomatricula	Section 5, Question 38: *If the answer is no, why did you not enrolled during the current school year?* (Age, lack of economic resources, work, domestic labour, studies completed, no interest, disease, lack of facilities or teachers, distance and/or transportation, pregnancy, other)
		tipoestablecimiento	Section 5, Question 39: *The establishment where you are enrolled is…* (Public; private; municipal or provincial council; or 'fiscomisional'[95])
	School Attendance	asistenciaescolar	Section 5, Question 40: *Do you normally attend the school where you are enrolled?* (yes or no)
	Educational Attainment	nivelinstruccion anioaprobado	Section 5, Question 41:

[95] The term "fiscomisional" relates to the financing sources of certain educational institutions. In this case, funding is shared by the government and a religious institution.

			What is the highest level of instruction that you achieved and in what year? (None, literacy centre, primary, basic general education, secondary, high school, postsecondary cycle, university, postgraduate)
	Enrolment in Child Development Centers (Children Under 5 Years Old)	asisteprogramainfantil programadesarrolloinfantil	Section 5, Question 28: *Does the child attend a public or private Development Centre?* (yes or no) Section 5, Question 29: *The development centre where the child attends is…* (public funded by MIES, public financed by other government institution, private, funded by the church, ONG financed)
	Health Status	presenciaenfermedad	Section 5, Question 21: *During the past month did you have an illness, accident, burn, toothache, ear pain or any other discomfort, although it has been momentary (except symptoms of pregnancy)?* (yes or no)
	Disability	discapacidad	Section 5, Question 18: *Do you have a permanent disability, such as blindness, paralysis, amputation, deafness, mentally retarded, insane, among others?* (yes or no)
	Frequency of Attendance at Health Centers for Medical Checkups (Children Under 5 Years Old)	frecuenciacontrol	Section 5, Question 26: *How often do you take the child to medical control?* (When she is sick, monthly, quarterly, semiannually, annually, never)

Poverty	Living Conditions	estadotecho	Section 3, Question 6a: *Condition of the roof* (good, regular, or bad)
		estadopiso	Section 3, Question 6b: *Condition of the floor* (good, regular, or bad)
		estadopared	Section 3, Question 6c: *Condition of the walls* (good, regular, or bad)
		propiedadvv	Section 4, Question 1: *The house occupied by the household is...* (own and fully paid; own and still paying; antichresis; leased; transferred or free; or received by services)
	Overcrowding	cuartos	Section 4, Question 5: *How many rooms has this household excluding kitchen, bathrooms, garages or those dedicated to business?* (number #)
		dormitorios	Section 4, Question 6: *Among these rooms, how many are used exclusively for sleeping?* (number #)
	Access to Basic Services	fuenteagua	Section 4, Question 7: *Where does the water come from?* (Public network; fountain or public tap; another pipeline source; delivery truck; pit; river or canal; rain water; other)
		tiposshh	Section 4, Question 10: *The type of toilet facility available is…* (sewage; septic tank; cesspool; direct download to ocean, river, lake or stream; latrine; or do not have)
		servicioducha	

		eliminabasura energeticocina	Section 4, Question 12: *The shower service available is…* (exclusive, shared, or do not have) Section 4, Question 13: *How is it eliminated most of the garbage?* (municipal service; thrown into the street or river; burned; buried; recycled; hired service; other) Section 4, Question 14: *What kind of lighting system has this household?* (public utility; private power generator; solar panel; candle/oil lamp/gas burner; or do not have) Section 4, Question 16: *In this household, food is cooked mainly using…* (Gas; wood/coal; electricity; other; or do not cook)
	Household Assets (television, car, telephone, etc.)		Section 6, Question 10a: *Does this household have a colour TV working?* (yes or no) Section 6, Question 10b: *Does this household have a refrigerator working?* (yes or no) Section 6, Question 10c: *Does this household have a stove working?* (yes or no) Section 6, Question 10d: *Does this household have a sound system working?* (yes or no)

			Section 6, Question 10e: *Does this household have a blender working?* (yes or no) Section 6, Question 10f: *Does this household have a telephone line working?* (yes or no) Section 6, Question 10g: *Does this household have a DVD working?* (yes or no) Section 6, Question 10h: *Does this household have a vehicle working?* (yes or no) Section 6, Question 10i: *Does this household have a washing machine working?* (yes or no) Section 6, Question 10j: *Does this household have a computer working?* (yes or no) Section 6, Question 10k: *Does this household have a microwave oven working?* (yes or no) Section 6, Question 10l: *Does this household have a food processor working?* (yes or no) Section 6, Question 10m: *Does this household have an iron working?* (yes or no) Section 6, Question 10n: *Does this household have an activated cell phone?* (yes or no)

			Section 6, Question 10o: *Does this household have Internet service?* (yes or no) Section 6, Question 10p: *Does this household have cable TV service?* (yes or no)
	Housing Characteristics	tipovv accesovv materialtechovv materialpisovv materialparedvv	Section 3, Question 1: *Type of house* (house, apartment, room(s), shack, or hut) Section 3, Question 2: *Main access road* (pavement, cobbled, dirt, path, river/sea, or other) Section 3, Question 3: *Main material of the roof* (concrete, asbestos, zinc, tile, palm/straw, or another material) Section 3, Question 4: *Main material of the floor* (parquet/plank/floating floor, tile/vinyl/ceramic, marble, cement/brick, wood, cane, soil, or another material) Section 3, Question 5: *Main material of the walls* (concrete, block/brick, asbestos/cement/fibrolite, adobe, wood, bahareque, cane/reed, or another material)
	Overall welfare*	SELBEN welfare index[96]	---

[96] The information collected by the Registro Social is used to create the SELBEN welfare index, which ranks Ecuadorian households according to their economic and structural characteristics. The score of a given household is computed using a nonlinear principal components analysis based on 27 variables, which include living conditions, household assets (television, car, telephone, etc.), access to basic services, human capital, socio-economic characteristics (employment, ethnicity, literacy, access to credit, etc.), and household size.

Geographic and Socio-economic Characteristics	Province	idprovincia	Section 1, Question 3 (24 provinces)
	Canton	idcanton	Section 1, Question 4 (221 cantons)
	Parish	idparroquia	Section 1, Question 5 (1,149 parishes)
	Census Area	zona	Section 1, Question 7
	Census District	sector	Section 1, Question 8 (32,129 districts)
	Housing Unit (Dwelling)	vivienda	Section 1, Question 12 (number #)
	Household Unit	hogar1 hogart	Section 1, Question 13 (number #)
	Employment Status	actividad	Section 5, Question 46: *What did you do last week?* (worked at least one hour; did not work but has a job; at least one hour manufactured a product or provided a service; at least one hour helped at some family business; at least one hour performed agricultural labour or took care of farming animals; 'unemployed' looked for a job having worked before and being available to work; did not work)
		alternativatrabajo	Section 5, Question 47: *If you have not worked…* (looking for a first time job and available for work; rentier; retired or pensioner; student; performs household duties; handicapped; other)
		tiempobuscatrabajo	Section 5, Question 48: *If unemployed, how long have you been looking for a job?* (less than a year; or more than a year)
	Employment Category	categoriaocupacion	Section 5, Question 51: *In this job you are working as…*(public

		tiponegocio mejoranegocio funcionanegocio	employee; private employee; labourer; patron; self-employed; unpaid home worker; unpaid worker in another home; unpaid assistant of a laborer; domestic employee) Section 5, Question 52: *If you are self-employed or patron, your business is…* (family, partnership, or individual business) Section 5, Question 53: *If you are self-employed or patron, what do you need to improve your business?* (credit; market access; assistance and training; other) Section 5, Question 54: *If you are self-employed or patron, where your business operates?* (in the place where you live; in its own place; in a leased place; in a borrowed or community-owned place; it moves from one place to another)
	Other Income Sources	reciberemesas	Section 6, Question 3: *In the last twelve months, have you received remittances from relatives or friends who are living abroad and are not household members?* (yes or no)
	Access to Credit	credito	Section 6, Question 4: *In the last twelve months, did any member of the household receive cash loans, credit cards or commercial credit to buy: furniture, domestic*

		usoprestamo	*appliances, clothing, education, housing, vehicles, among others?* (yes or no) Section 6, Question 5: *How did you mainly use the loan or credit?* (housing purchase, construction or remodeling/purchase of land; health; vehicle; appliances and furniture; travel; food; studies; business; debt payment; or others)
Demographic Characteristics	Gender	genero	Section 5, Question 3 (male or female)
	Age	edad	Section 5, Question 4: *How old are you?* (number #)
	Kinship	parentescojefe	Section 5, Question 7: *What is the relationship of each household member with respect to the head of household?* (Head of household, spouse or partner, child, stepchild, parent, father in law, son or daughter in law, grandchild, brother, brother in law, other relative, not relative, or domestic service)
	Marital Status	estadocivil	Section 5, Question 8: *What is the current marital status of each household member?* (Free Union, married, widowed, separated, divorced, or single)
	Nationality	nacionalidad	Section 5, Question 12: *What nationality is each member?* (Ecuadorian, Colombian, American, Peruvian, Spanish, Cuban, Venezuelan, or another nationality)

	Ethnicity	etnia	Section 5, Question 13: *What ethnic group do you consider yourself?* (Indigenous, Afro-Ecuadorian, black, mulatto, montubio, mestizo, white, other)
Engagement in Social Assistance Programmes	CDH	cdh gastosaludcdh gastoeducacioncdh equipamientocdh gastopagodeudacdh gastoalimentacioncdh negociocdh otrosgastoscdh	Section 5, Question 61: *Have you received the CDH?* (yes or no) Section 5, Question 62a: *Did you use the money for health expenses?* (yes or no) Section 5, Question 62b: *Did you spend the money in education?* (yes or no) Section 5, Question 62c: *Did you spend the money in household equipment?* (yes or no) Section 5, Question 62d: *Did you use the money to pay debts?* (yes or no) Section 5, Question 62e: *Did you spend the money in food?* (yes or no) Section 5, Question 62f: *Did you spend the money in your business?* (yes or no) Section 5, Question 62g: *Did you spend the money in personal expenses?* (yes or no)
	BDH	bdh	Section 5, Question 58: *Have you been a beneficiary of the BDH?*[97] (yes or no)

[97] Social assistance transfers for older adults and people with disabilities are also included.

		gastosaludbdh	Section 5, Question 59a: *Do you use the BDH for health expenses?* (yes or no)
		gastoeducacionbdh	Section 5, Question 59b: *Do you use the BDH for education?* (yes or no)
		equipamientobdh	Section 5, Question 59c: *Do you spend the BDH in household equipment?* (yes or no)
		gastopagodeudabdh	Section 5, Question 59d: *Do you use the BDH to pay debts?* (yes or no)
		gastoalimentacionbdh	Section 5, Question 59e: *Do you spend the BDH in food?* (yes or no)
		otrosgastosbdh	Section 5, Question 59f: *Do you use the BDH for other personal expenses?* (yes or no)
		tiempolugarbdh	Section 5, Question 60: *Approximately how long does it take you to get to where the BDH is handed?* (less than 30 minutes; 30 minutes; 45 minutes; 1 hour; or more than an hour)

Appendix D: Relationship Among the Probability, Odds and Log of Odds

The transformation from probability to odds is a monotonic transformation, meaning the odds increase as the probability increases or vice versa. Probability ranges from 0 to 1, while odds range from 0 and positive infinity. The transformation from odds to log-odds is simply a log transformation, which is again monotonic. The table below shows the relationship among the probability, odds and log of odds.

Probability (p)	odds	log-odds
.001	.001001	-6.906755
.01	.010101	-4.59512
.15	.1764706	-1.734601
.2	.25	-1.386294
.25	.3333333	-1.098612
.3	.4285714	-.8472978
.35	.5384616	-.6190392
.4	.6666667	-.4054651
.45	.8181818	-.2006707
.5	1	0
.55	1.222222	.2006707
.6	1.5	.4054651
.65	1.857143	.6190392
.7	2.333333	.8472978
.75	3	1.098612
.8	4	1.386294
.85	5.666667	1.734601
.9	9	2.197225
.999	999	6.906755
.9999	9999	9.21024